

UNIVERSITY OF TORONTO



3 1761 00052025 4

Digitized by the Internet Archive
in 2007 with funding from
Microsoft Corporation



573.
I
PRINCETON CONTRIBUTIONS
///

TO

PSYCHOLOGY.

(REPRINTED FROM THE PSYCHOLOGICAL REVIEW AND
OTHER JOURNALS.)

v. 1-2 (1895-98)

EDITED BY

J. MARK BALDWIN,

*Stuart Professor of Psychology
Princeton University.*

(VOL. I—1895-6.)

PRINCETON, N. J.
THE UNIVERSITY PRESS.

BF

21

P8

v. 1-2

[*For Table of Contents, see p. 183.*]

656318

16 . 4 . 57

TABLE OF CONTENTS.

VOL. I.

	Page.
I. <i>General Introduction: Psychology Past and Present:</i> J. MARK BALDWIN, - - - - -	1
II. <i>Freedom and Psychogenesis:</i> A. T. ORMOND, - - -	31
III. <i>Studies from the Princeton Psychological Laboratory (I-V):</i>	
I. <i>Memory for Square Size:</i> J. MARK BALDWIN and W. J. SHAW, - - - - -	45
II. <i>Further Experiments on Memory for Square Size:</i> H. C. WARREN and W. J. SHAW, -	48
III. <i>The Effect of Lize-Contrast upon Judgments of Position in the Retinal Field:</i> J. MARK BALDWIN, - - - - -	53
IV. <i>Types of Reaction:</i> J. MARK BALDWIN, assisted by W. J. SHAW, - - - - -	68
V. <i>Sensations of Rotation:</i> H. C. WARREN, -	82
IV. <i>Sensory Stimulation by Attention:</i> J. G. HIBBEN, - -	87
V. <i>The Perception of Two Points not the Space Threshold:</i> GUY TAWNEY, - - - - -	95
VI. <i>The Origin of a 'Thing' and its Nature:</i> J. MARK BALDWIN, - - - - -	105
VII. <i>Something More about the 'Prospective Reference' of Mind:</i> W. M. URBAN, - - - - -	129
VIII. <i>Genetic Studies (I-II):</i> J. MARK BALDWIN:	
I. <i>Consciousness and Evolution,</i> - - - - -	145
II. <i>A New Faction in Evolution,</i> - - - - -	155
IX. <i>Table of Contents of Vol. I.,</i> . - - - -	183

I. GENERAL INTRODUCTION: PSYCHOLOGY PAST AND PRESENT.

BY PROFESSOR J. MARK BALDWIN,

Princeton University.

I. HISTORICAL.

Modern psychology has had its principal development in Great Britain, Germany, and France. Germany has undoubtedly had greatest influence in this movement, considered in all its branches. The two main currents of development previous to the rise of the new so-called 'scientific' psychology, designated as 'speculative' and 'empirical,' had their initial impulse, as well as their fruitful pursuit, respectively in Germany and Britain. German psychology down to the rise of the Herbartian movement was a chapter of deductions from speculative principles; English psychology was a detailed analysis of the experiences of the individual consciousness. Kant, Fichte, and Hegel may sufficiently represent the succession in Germany; James Mill, John Stuart Mill, Hume, Reid, and Bain, that in Great Britain.

The work of Herbart and his school tended to bring a more empirical treatment into German thought, and its significance was twofold: it excited opposition to the speculative method, and it prepared the Germans for the results of English analysis. It is further a legitimate supposition that the spirit of experimental inquiry which has swept over Germany in this century was made more easily assimilable by workers in this department, also, by the patient and extraordinary attempt of Herbart to construct a 'mechanic' and 'static' of mind in his '*Psychologie als Wissenschaft*' (1824).

* Part of the material to be used in different form in a 'Historical and Educational Report,' prepared by the author (by request) in his capacity as 'Judge of Award' for this subject at the World's Columbian Exposition.

To German thinkers also belongs the credit due to originators of all new movements which show their vitality by growth and reproduction, in that the experimental treatment of the mind was first advocated and initiated in Germany. But of this I write more fully below.

The contribution of France to psychology has been decidedly of less importance; yet the work of its writers has also illustrated a fruitful and productive movement. It has been from the side of medicine that French work has influenced current wide-spread conceptions of consciousness. Mental pathology and the lessons of it for the theory of the mind have come possibly most of all from France; or at any rate—not to disparage the admirable recent work of English and German investigators—the tendency, so to speak, of the French treatment of consciousness has been to approach mental operations from the abnormal side.

In America the influences which have tended to control psychological opinion have been mainly theological on one side and educational on the other. The absence of great native systems of speculative thought has prevented at once the rationalistic invasions into theology which characterized the German development, and the attempts at psychological interpretation which furnished a supposed basis of fact to the idealistic systems. In Germany various 'philosophies of nature' sought to find even in objective science support for theoretical world-dialectic: and psychology fared even worse, since it is, *par excellence*, the theatre for the exploitation of universal hypotheses. But in America men did not speculate much: and those who did were theologians. So naturally the psychologists were theologians also. Jonathan Edwards had a doctrine of the agent because free-will was a question of theology.

The educational influence was auxiliary merely to the theological. The absence of large universities with chairs for research; the nature of the educational foundations which did exist under denominational control; the aim of education as conceived in the centres where the necessity for supplying growing towns with pastors was urgent; the wholesome fact for our civilization that the Puritans had traditions in favor

of the school and the religious school—all these things made it only necessary that books sound in their theological bearings, or affording homiletic lessons in living, should be written, in a topic of such central importance. Even the term 'psychology' is only now getting domesticated: 'mental' and 'moral' philosophy were the titles of courses of instruction on the 'soul.'

The type of philosophy which these conditions encouraged was, it may easily be imagined, realistic; and it is probably for the reasons which I have indicated that the Scottish Natural Realism was the American type of thought, and is now, except in the great university centres where systematic philosophy has become an end in itself apart from its duty to theology and education. As far as psychology was concerned, this realistic tendency was a great good. It led to a magnification of mental reality, to a reverence for the 'utterances of consciousness,' to a realistic interpretation of the 'immediate knowledge of self,' to the firm settling of the great 'intuitions,' cause, time, space, God, etc.; and in as far as this led to the direct examination of consciousness and to the testing of philosophical claims by consciousness, it prepared the way for a better and broader method. This tendency is marked even in the more influential works in theology. Channing and Emerson no less than Smith and Charles Hodge lay the corner-stone of argument again and again in the proof 'from consciousness.'

This tendency to a psychological view of philosophy and its basis in the religious motive is seen also in Scotland, the home of realism: and it is there a part of the British method of thought which I have already spoken of. The works on psychology written in America up to 1880 were, as we should expect, from the hands of theologians and educators, usually both in the same person; for it is a further proof of the association of psychology and theology that the mental and moral philosophy in the colleges was almost without exception put in the hands of the president of the college, and he was by unanimous requirement a preacher. So were written a series of works which are landmarks of American scholarship, props of evangelical theology, disciplinary aids of the highest value

to the growing student, and evidences—to revert again to my argument—of the twofold influence I have indicated. Edwards's 'Freedom of the Will' (1754), Tappan's 'Review of Edwards' (1839) and 'Doctrine of the Will determined by an Appeal to Consciousness' (1840), Hickok's 'Rational Psychology' (1848) and 'Empirical Psychology' (1854), Porter's 'Human Intellect' (1868) and 'Moral Science' (1885), McCosh's 'Psychology' (1887) and 'First and Fundamental Truths' (1889)—these and other books like them show the psychology of America up to about 1880.

Speaking for psychology alone, not for philosophy, it is easy to point out their merits and defects, not in my individual judgment, but as compared with the standards of the present year of the Exposition. It is necessary, however, rather to show this by sketching the present and showing the new elements which have modified the American work and whence they came.

Coming to the present state of psychological thought, my task is made easier by reason of the divorce which has been forced between psychology as a science on one hand and metaphysics on the other. As was said above, Herbart, while failing in his attempt to apply mathematics to mental 'permutations and combinations,' yet prepared the way for a new treatment of mental phenomena. After his attempt it began to be seen that the facts of conscious life were first in order of importance and were capable of treatment in a detailed way quite independently of the questions of Being, the Absolute, and the like. The works of Volkman, 'Lehrbuch der Psychologie' (4th ed., 1894), and Lipps, 'Die Grundthatsachen des Seelenlebens' (1883), illustrate this.

This was only to begin to do what had been doing in England since Locke. But the Germans now went further: they asked the question—which had been groped upon before by Descartes, by Leibnitz, and by Reid—how can psychology be a science when one of the evident conditions of the flow of mental states, of their integrity and their trustworthiness, the brain, is left quite out of account? What is the law of connection of mind and brain? And is it possible to modify the brain and so to modify the mind? If so, then that great in-

strument of scientific work, experiment, may perform a part for the psychologist also, and his resources be magnificently enlarged.

This is the question of Experimental Psychology. It was answered in Germany in the affirmative. Lotze, in my view, deserves the credit of it, the credit of the great-minded constructive pioneer; and Wundt is the founder of the science in the sense that he first realized the expectations of Lotze's genius by actually planning and executing experiments on a large scale which made the affirmative answer an irreversible fact of history. Lotze's '*Medicinische Psychologie*' appeared in 1852, Wundt's '*Grundzüge der Physiologischen Psychologie*' in 1874. Between the two, however, came Fechner, whose theoretical construction of the new work and its methods shows all the exactness of treatment of similar discussions of natural-science principles by electricians and chemists, and published the formulas in which he attempted to give universal statement to the discoveries of E. H. Weber on the intensity of sensation-states. Fechner's '*Elemente der Psychophysik*' appeared in 1860.

Apart from the actual development of this new method—a point to be spoken of later on—it has profoundly modified the general conception of psychology even where its validity as a method has been denied. There has been nothing less than a revolution in the conception of psychology since the publication of the works just named. One of the motives of this revolution came thus from Germany. The other—for it has two great phases—is due to English thinkers: the evolutionists, of whom Herbert Spencer ('*Principles of Psychology*,' 1855) is the chief. These two influences are seen in two great points of contrast easily made out between the psychology of to-day and that of yesterday in America. The latter I have described above. Its two main characteristics, for purposes of the present contrast, are first, its character as so-called '*faculty-psychology*'; and second, its character as holding to what I may call a '*ready-made*' view of consciousness—technically an '*intuition*' view of consciousness. In opposition to these characters, current psychology is '*functional*'—holding to mental '*functions*' rather than to mental faculties; and finds this

function to be 'genetic' rather than intuitive — the functions 'grow,' instead of being 'ready-made.'

The old conception of 'faculties' made the different phases of mental process in large measure distinct from one another. Memory was a 'faculty,' a 'power' of the mind; thought was another, imagination a third. The new functional conception asks how the mind as a whole acts, and how this one form of activity adapts itself to the different elements of material which it finds available. The old terms 'memory,' 'thought,' etc., are retained; but with the distinct understanding that they do not stand for divisions in the mind, or different processes, one of which may be held in reserve when another is acting, etc. On the contrary, the process in consciousness is one; and it is a psycho-physical process as well. The particular way in which this one function shows itself is a matter of adaptation to the changing conditions under which the activity is brought about. This transition is due in part also to the insight of Herbart and to the demand for unity insisted upon by the evolutionists.

The other point of contrast is equally plain. The 'genetic' point of view in current discussion is opposed to the older 'intuitive' point of view. The mind is looked upon as having grown to be what it is, both as respects the growth of the man from the child, and as respects the place of man in the scale of conscious existences. The understanding of mental facts is sought in the comprehension of their origin as well as their nature: and the question of the validity or worth of 'intuitive' beliefs in consciousness is subordinated to the question as to how the mind came to have such beliefs.

Both of these points of contrast have been further defined by the progress of general philosophy in America. The demand for unity in mental interpretation has not come from naturalistic evolution alone (John Fiske, 'Outlines of Cosmic Philosophy,' 1874; Thompson, 'System of Psychology,' 1884); an equally pressing demand has come from idealistic metaphysics, which seeks for continuity in the natural series as zealously as does the advocate of evolution. The influence of Hegel, as interpreted in the works of Green, and later in those of Caird, has been potent in effecting this transformation. It

is easy to see also that the same union of forces is quite feasible as respects the genetic development of consciousness, although the new idealists have not done justice to this growing tendency in modern psychology.

The line of cleavage, in the current discussions of general psychology, is drawn on the question of the interpretation of mental 'function': both sides claiming the same full liberty of genetic research and the same resources of analysis and experiment. The 'Associationists,' on one hand, carrying on the tradition of the British empiricists, construe mental function after analogy with the ordinary interplay of forces in the objective world; the 'Apperceptionists,' on the other hand, hold that mental function is a form of irreducible cosmic process. Apart from original monographs on special topics, no work on psychology to-day commands much attention, either from psychologists or from students of philosophy, which does not show itself alive to this main issue. The works of Lotze and Wundt have had great influence upon Americans in the direction of this general statement of the problems of psychology: and it is especially the philosophy of Lotze which is replacing by a reasoned and critical realism the earlier theological dogmatic view so long prevalent in the United States by inheritance from Scotland.

On the literature of present-day psychology I can do no better than quote the following passage freely translated from the most recent German work on general psychology, itself fully representative of the present state of knowledge—'Grundriss der Psychologie,' by Professor Külpe of the University of Leipzig (pp. 27 ff.):

"About the middle of the nineteenth century experimental and psycho-physical psychology began its course in Germany. While Herbart recognized a threefold influence of the body upon the mind, . . . it was Lotze who made a thorough beginning in the employment of the data of physiology. Lotze, indeed, began his work with certain metaphysical expositions after the manner of the older German writers, and is very far from the recognition of a universal psycho-physical parallelism. But he does not hesitate to speak of the nervous conditions of mental processes, and he had the good fortune to suggest

hypotheses of value where exact knowledge was wanting. The real foundation of Experimental Psychology was laid, however, by G. T. Fechner, who sought to carry out in a thorough-going way the conception of a functional relation between mental and physical processes. Although the mathematical form which he gave to this relation . . . does not hold, yet he gave to the exact science of psychology an extraordinary impulse, by reason of the new conceptions which he introduced, the methods of procedure which he both formulated and applied, the working over which he gave to the material he had in hand, and the observations and researches which he himself carried out. . . . The union of the experimental and psycho-physical was finally accomplished by Wilhelm Wundt . . . in his classical '*Grundzüge der Physiologischen Psychologie*' (1874, 4th ed. 1893). By this unity of conception and by his comprehensive treatment of all mental phenomena . . . he has made the current phrase 'modern psychology' applicable. . . . Wundt gave a further important impulse to the cultivation of experimental psychology by founding the laboratory in Leipzig in 1879, and establishing the '*Philosophische Studien*,' a journal devoted mainly to the publication of researches from his institute.

"Additional works may be mentioned of very recent date, which must be reckoned in their character as belonging to the modern psychology thus founded by Wundt, although they differ more or less essentially in system and in theory from him and from one another: Höffding, '*Psychologie in Umrissen*,' 2d ed., 1893, German translation from the Danish (English translation, 1891); Ladd, '*Elements of Physiological Psychology*,' 1887; Sergi, '*La Psychologie Physiologique*' (translation from the Italian, 1888); W. James, '*The Principles of Psychology*,' 1890; Ziehen, '*Leitfaden der physiologischen Psychologie*' (1891; 2d ed., 1893); Baldwin, '*Handbook of Psychology*,' 1891 (2d ed.; 1st ed., 1889-90); J. Sully, '*The Human Mind*,' 1892.

"We may mention also certain periodicals which represent the same current of psychological thought: '*Philosophische Studien*,' edited by W. Wundt (vols. 1-8, 1883 ff.); '*The American Journal of Psychology*,' edited by G. S. Hall (vols.

1-5, 1887 ff.); 'Zeitschrift für Psychologie und Physiologie der Sinnesorgane,' edited by H. Ebbinghaus and A. König (vols. 1-5, 1890 ff.)."

The part taken by American students in the present psychological movement is seen in the fact that of the seven works thus cited by Külpe three are by Americans, and to them must be added 'Psychology: Descriptive and Explanatory' (1894), by G. T. Ladd, and the journal 'The Psychological Review,' edited by J. McK. Cattell and J. Mark Baldwin (vol. 1, 1894). Another important French work of recent date is 'La Psychologie des Idées-Forces,' by A. Fouillée (1893). The position of psychology in the American colleges and universities is described in a further section below.

Other important contributions to Experimental Psychology—apart from the long series of monographs and research articles published in Germany and America—are Helmholtz, 'Physiologische Optik' (1867, 2d ed. f. 1886, French translation), and 'Tonempfindungen' (1863, English translation); Stumpf, 'Tonpsychologie' (1883-90); and Münsterberg, 'Beiträge zur experimentellen Psychologie,' Parts I-IV (1889-93).

The contribution from the side of mental pathology has become important on account of the *rapprochement* which has obtained in recent years between the alienist and the psychologist. The works of Pierre Janet, 'Automatisme psychologique' (1889) and 'L'État mental des Hystériques' (1892-93), and of Bernheim, 'Suggestive Therapeutics' (English translation, 1889), and 'Études de la Suggestion' (1892), are most important. To them should be added the works of Ribot, 'Diseases of the Will,' English translation (5th French ed., 1888); 'Diseases of Memory,' English translation (5th French ed., 1888); 'Diseases of Personality' (2d ed., 1888; English translation, 1891), together with the many original contributions on the subject of hypnotism and aberrations of personality published in the 'Revue Philosophique' (edited by Th. Ribot, vols. 1-xxxvi, 1876 ff.) and summed up in part in 'Les Altérations de la Personnalité' (1893) of Alf. Binet.

Further, the treatment of psychology in accordance with the British tradition, from the point of view of description and analysis, has been carried forward by Ward in the article

'Psychology' in the *Encyclopædia Britannica*, 9th ed. This type of research has also had its organ of publication in '*Mind: a Journal of Psychology and Philosophy*,' edited by G. Croom Robertson (vols. I-XVI, 1876 ff.) and by G. F. Stout (New Series, vols. I-III, 1892 ff.).

Finally, the genetic treatment of consciousness has been advanced by the works of Spencer, '*Principles of Psychology*,' 1855 (3d ed., 1880); Romanes, '*The Origin of Human Faculty*,' 1884-1888; Morgan, '*Animal Life and Intelligence*' (1891); and Galton, '*Inquiries into Human Faculty*' (1883) and '*Natural Inheritance*' (1889).

II. THE METHOD AND MAIN DIVISIONS OF EXPERIMENTAL PSYCHOLOGY.

To say that this is the age of science is only to repeat what is now trite and what no student either of philosophy or of history needs to be told. It is the age of science because it is the age of devotion to science and of results in science. But it is a very different thing to say that this is the age of scientific method. Former ages have seen devotion to science and results in science, but I venture to say that no former age has, as an age, realized a scientific method. So prevailing, however, has the new method now become, and so customary to us, that it is only by historical study that we are able either to see that it is new, or to work ourselves into that degree of intellectual sympathy for the old which the earnest endeavor and unflagging patience of the heroes of philosophy in the past rightfully demand for all time.

In characterizing our time by the word 'scientific,' as regards method, I mean to say something which is true in philosophy, politics, literature, as well as in the investigation of nature; and to dwell only on the department of thought in which such a method has been, and is, most difficult to realize. In philosophy it is not fully realized; and yet I believe that any class or school of philosophic thinkers who do not face toward the scientific east are steering up-current and will be absent when science and philosophy enter a common barge and together compass the universe of knowledge. For it is a

part of the same conviction as to scientific method that neither science nor philosophy will ever succeed in compassing it alone. However painfully this advance may have been won and however loudly the dogmatists may deny its justification, it is sufficient here to signalize the fact that philosophy has in the present half-century thrown open her doors to the entrance of critical and empirical methods, and that the results already accruing are evidence of the bigness of her future harvest.

In general philosophy what has been called scientific method is better known, as I have said above, in a twofold way, as empirical and critical. Retrospectively what we now have to rejoice in in philosophy is due about equally to two traditions, represented by Hume and Kant. The burden of current idealism, as far as it is worthy of consideration in our time, is to purify and conserve the work of Kant. And the burden of empiricism, under the same restriction, is to refute Kant with the only weapons which he himself considered of worthy temper. The battle is drawn at these close quarters, and round them both is thrown a common ring of scientific procedure.

In psychology the modern transformation comes most strongly out. Here we find an actual department of knowledge handed over to a new class of men for treatment, so remarkable is the demand for scientific method. It is no longer sufficient that a psychologist should be familiar with general philosophy and its history, or capable of acute logical criticism of systems; it is necessary, if he would deal successfully with the new problems and gain the ear of the advanced philosophical public, that he should reason from a basis of fact and by an inductive procedure. In short, he must not bring his philosophy as speculation into psychology, but must carry his psychology as fact, in its connection with physiology; ethnology, etc., into general philosophy.

To illustrate this change, and its effect on general theories, recent discussions of the idea of space may be cited in comparison with its earlier and more speculative treatment. The reasonings of James, Wundt, Bain, Spencer, differ so essentially from the argumentation of Kant and earlier men that it is almost impossible to find common ground between them. No one among those who accept Kant's results depends in our

day very largely upon his reasons: the question is shifted to another field. The physiologist has as much to say about it to-day as the psychologist, and the speculative philosopher must recognize them both.

This tendency of the day in philosophy may be expressed by a chemical figure as a 'precipitating' tendency. We are endeavoring, and successfully too, to throw all questions which are capable of such treatment to the bottom, as a precipitate—a psychological precipitate—and are then handing them over to the psychologist for positive treatment. As long as our data remained in a solution of ninety parts water (which, being interpreted, means speculation), it was difficult to handle them scientifically. While admitting the utility and necessity of ontology in its place, current psychology claims that its place must be better defined than formerly it has been, and that whenever we can secure a sediment, a residuum, a deposit, apart from a speculative solvent, this is so much gain to positive science and to truth.

One of the ideas which lie at the bottom of the so-called 'new psychology' is the idea of measurement. Measurement, determination in quantity and time, is the resource of all developed science, and as long as such a resource was denied to the psychologist he was called a scientist only in his function of description and classification, not in the more important functions of explanation and construction. And the justification of the application of measurement to psychological facts has come, not from theoretical considerations—for they were all opposed, and still are, in many of the books of the new idealism—but from practical attempts to do what philosophy declared to be impossible. That is, experiment has been the desired and only 'reagent.' It is true that theoretical justifications are now forthcoming of the application of experiment to consciousness, but they are suggested by the actual results and were not in sufficient currency to hinder the influence of Kant's ultimatum, for example, that a science of psychology was impossible.

By experiment in this connection is meant experiment on the nervous system with the accompanying modifications it occasions in consciousness. Efforts have been made in earlier

times to experiment upon states of consciousness directly. Descartes deserves credit for such efforts, and for the intimation he gives us, in his theory of emotions, of an approach to mind through the body. But the elevation of such an approach to the place of a recognized psychological method was not possible to Descartes, Kant, or any one else who lived and theorized before the remarkable advance made in this half-century in the physiology of the nervous system. And even as it is, many questions which will in the end admit of investigation from the side of the organism are still in abeyance till new light is cast upon obscure processes of the brain and nerves.

A little further reflection will show us that the employment of experiment in this sphere proceeds upon two assumptions which are now generally admitted and are justified as empirical principles, at least by the results. They are both assumptions which the physical scientist is accustomed to make in dealing with his material, and their statement is sufficient to exhibit their elementary importance, however novel they may sound to those who are accustomed to think and speak of mind as something given to us in entire independence of organic processes. The first of these assumptions is this: that our mental life is always and everywhere accompanied by a process of nervous change. This is seen to be necessary to any method which involves the passage of mind to body or the reverse by the interpretation of effects. Which is cause and which effect, the mental or the physical change, or whether they both are effects of an unknown cause, is immaterial—to consider such a question would be to introduce what I have called the ‘speculative solvent.’ It is sufficient to know that they are always together, and that the change in one may be indicated in symbols which also represent the change in the other. The second assumption is based upon the first, viz., that this connection between mind and body is uniform. By this is meant what in general induction is called the uniformity of nature. Any relation sufficiently stable to admit of repeated experiment in the manipulation of its terms is in so far uniform. Experiment would be useless if the relation it tends to establish were not stable, since the result of such experiment would give no antecedent likelihood as to the result of others under similar

circumstances. Experimental psychology, therefore, rests upon the assumption that a relation of correspondence—be it coexistence or causation—once clearly made out between a mental and a nervous modification, it must hold good under any and every repetition of the same experiment under the same conditions.

These two assumptions made, we have at once the possibility of a physical approach to the facts of consciousness. The result is a relative measurement of such facts in terms of the external stimulation of the nerves, in regular and normal conditions of the activity of attention.

Further, it is apparent that such a means of experimentation may become available either under artificial or under natural conditions, according as the nervous stimulation is due to an external excitation, or arises from some unusual condition of the organism itself. All cases of brain or nervous disease, on the one hand, offer opportunities for boundless observation; the unusual manifestations being changes due to the organic disturbances of disease. Here nature has arranged and actually performed the experiment for us; the only difficulty being the physiological one, that the cerebral states may be as obscure as the mental states which they are used to explain. All such cases of mental changes due to internal organic changes are classed together under the name of Physiological Psychology. It includes all questions which relate to nerve physiology and pathology, illusion, hallucination, mental disease, hypnotism.

On the other hand, experiments may be arranged for the normal stimulation of the sense-organs—skin, muscles, special senses—under artificial conditions as explained in part below. This is Experimental Psychology. On these lines modern experimental psychology falls into two great departments. As the normal properly precedes the abnormal, it is well to consider the line of researches based upon external experiment, confining ourselves to a more or less cursory view of results of historical interest.*

* In the official report, sections are included on 'Psycho-physics' (Weber's Law) and 'Mental Chronometry' (Reaction-times).

III. THE EXHIBITS IN PSYCHOLOGY AT CHICAGO.

We are now prepared to consider the exhibits made in the interests of Experimental Psychology at the Columbian Exposition. It is evident that departments in which progress is in the main abstract and immaterial—such as the social, moral, and theoretical sciences—cannot show their work to the eye, and so have heretofore appeared at the world's great expositions only as their results have been embodied in more practical life, in education, and in institutions. It is, however, unfortunate that this should be so: for the more ideal and spiritual aspects of a nation's life are just the aspects in which popular instruction is defective, and these are the aspects which should least of all be omitted in a survey of the conditions of present-day civilization. Yet it is so; and it becomes easy to see, therefore, that it is only as psychology has become experimental and so has found it possible to state her problems and results to a degree in forms which allow of diagrammatic and material representation that she is able to 'exhibit' herself. What psychology showed, therefore, at the Chicago Exposition was the experimental side, as I have sketched its problems and methods in what precedes.

The exhibits bearing on psychology in its scientific aspects—as apart from the educational aspects, of which I speak later on—may be placed in order thus:

(A) A collected exhibit made by the department of Anthropology, of which Professor F. W. Putnam of Harvard University was chief, under the immediate direction of Professor Joseph Jastrow of the University of Wisconsin, consisting of a Psychological Laboratory in operation with all its accessories.

(B) A collection of instruments shown in the German Educational Exhibit under the heading 'Psychophysics.'

(C) Instruments shown in the general exhibit of the 'Deutsche Gesellschaft für Mechanik und Optik.'

(D) The private exhibits of particular instrument-makers.

(E) Exhibits made by single universities, i.e., those by the University of Pennsylvania and the University of Illinois.

I may consider these briefly in order.

(A) *The Laboratory for Experimental Psychology, gathered by the Department of Anthropology (Ethnology).*—This laboratory constitutes the first attempt ever made to exhibit at an international fair the state of progress of the world in this branch. When taken in connection with the other laboratories exhibited by this department, i.e., in Anthropology and Neurology, it may be accepted, in its main features, as an adequate historical index of the psychological progress of the nineteenth century. The general features of the working laboratory cannot be better described than in the words of the director, Professor Joseph Jastrow.*

The Psychological Laboratory.—"The object of this laboratory is to illustrate the methods of testing the range, accuracy, and nature of the more elementary mental powers, and to collect material for the further study of the factors that influence the development of these powers, their normal and abnormal distribution, and their correlation with one another. The laboratory is thus designed, not as are those connected with universities, for special research, or for demonstrations and instruction in psychology, but as a laboratory for the collection of tests. As in physical anthropometry the chief proportions of the human body are systematically measured, so in mental anthropometry the fundamental modes of action upon which mental life is conditioned are subjected to a careful examination. In both cases the first object is to ascertain the normal distribution of the quality measured. With this determined, each individual can find his place upon the chart or curve for each form of test and from a series of such comparisons obtain a significant estimate of his proficiencies and deficiencies. It should not be overlooked that mental tests of this kind are burdened with difficulties from which physical measurements are comparatively free. Our mental powers are subject to many variations and fluctuations. The novelty of the test often distracts from the best exercise of the faculty tested, so that a very brief period of practice might produce a more constant and significant result. Fatigue and one's physical condition are also important causes of variation. It is im-

* Official Catalogue of Exhibits, Department M, in which full descriptions may be found.

possible in the environment of the present laboratory to secure the necessary time and facilities for minimizing these objections. They detract more from the value of an individual record than from that of the combined statistical result. So much remains to be done in this line of investigation that at every step interesting problems are left unanswered. But what has been done emphasizes the importance and probable value of further research. The problems to be considered when once the normal capacity has been ascertained are such general ones as the growth and development with age of various powers; what types of faculty develop earlier and what later; how far their growth is conditioned upon age and how far upon education; again, the difference between the sexes at various ages, differences of race, environment, social status, are likewise to be determined. The relation of physical development to mental, the correlation of one form of mental faculty with others, the effect of a special training, —these, together with their many practical applications, form the more conspicuous problems to the elucidation of which such tests as are here taken will contribute. In addition to the interest in his or her own record, the individual has thus the satisfaction of contributing to a general statistical result."

(B), (C), (D), (E) *The Exhibits of (B) the German Educational Department, (C) the 'Deutsche Gesellschaft für Mechanik und Optik,' (D) Individual Private Instrument-makers, and (E) Separate Universities.*—The two German agencies mentioned as (B) and (C) send what may be considered as on the whole the best indication—when taken in connection with the special pieces of apparatus sent from German workshops to the collective exhibit of the department of Anthropology—of the application of modern mechanical skill to the construction of instruments of the delicacy required for psychological experiment. These instruments are mainly adaptations of well-known principles, and often of well-known apparatus, used in experimental physiology, physical optics and acoustics, electricity, etc. The instruments shown by the German Mechanical and Optical Society are almost entirely common to psychology and these sciences. The pieces in the German Educational Exhibit are largely the special arrangements found useful in the labora-

tory at Leipzig, and so show very inadequately the real progress of the science in Germany. Yet they are of great historical interest. The collection is much less complete than that made by the German instrument-makers in connection with the collective exhibit in the Department of Anthropology. In this connection it should be mentioned that the account given of Experimental Psychology in Germany by Professor Wundt in the official book, '*Die deutschen Universitäten*' (ed. by W. Lexis, 1893), is not adequate if considered (and probably the author did not intend it to be so considered) as an exponent of the present condition of this science and the place it occupies in the German universities.

(D) The private exhibits of individual firms should be noted in the attempt to make one's conception of psychological activity complete. French exhibitors did not combine as the Germans did, and so lost both in effect and in local position. Yet much of the finest work is done in Paris, as is witnessed by the cases of surgical, physical, and psychological instruments grouped in the north end of the Anthropology building. An examination of the catalogues of the exhibitors (for example, that of Ch. Verdin of Paris) may serve for the details of this class of exhibits, as the united catalogues of the other collections mentioned serve in respect to them. The German makers have done their work more largely in connection with great university laboratories, and so have subserved better the needs of particular students in solving particular problems in physics and psychology: the French, on the other hand, have found the demand more marked from the side of clinical medicine and experimental physiology.

(E) The separate university exhibits of the Universities of Pennsylvania and Illinois were located respectively in the Liberal Arts and the Illinois State building. The aim of the former was to present a working laboratory restricted to a small number of topics. This original purpose was not subserved through the failure to provide attendants to collect experimental data; yet the arrangements for experiments in reaction-times and the visual æsthetics of form were instructive to visitors. Two pieces of new apparatus were exhibited by Dr. Lightner Witmer, the designer of them: a complex

color-wheel which permits the alteration while in motion of the proportion of colors mixed, and a graphic movement apparatus involving new features.

The exhibit of the University of Illinois was mainly of instruments which were also included in the main collection of the Department of Ethnology. It was in charge of Professor W. O. Krohn of that university.

IV. EDUCATIONAL.

The educational aspects of the new work in psychology are of great importance. It is evident that education has two claims to make upon this study; one of these claims the old psychology aimed to meet, the other it was incapable of meeting. The first of these two duties of psychology to education is this: it should take its place as a factor in liberal collegiate culture in both of the functions which a great branch of learning serves in the university curriculum, i.e., undergraduate discipline and instruction, and post-graduate research discipline.

The older psychology, especially in America where it was hampered by the conditions pointed out in an earlier section, did, as I say, aim to instruct undergraduates. But even in this it was a means to another end: it was propædæutic to a philosophy and to a theology, both of which, as far as their demands upon 'mental science' were concerned, were dogmatic and intolerant. But the graduate disciplinary function was never served in any sense by the faculty psychology nor by the philosophy founded upon it in America.

The second great educative function of psychology is this: it should mould and inform educational theory by affording a view of mind and body in their united growth and mutual dependence. Education is a process of the development under most favorable conditions of full personality, and psychology is the science which aims to determine the nature of such personality in its varied stages of growth, and the conditions under which its full development may be most healthfully and sturdily nourished. One of the first duties of psychology, therefore, is to criticise systems of education, to point out 'the

better way' in education everywhere, and to take no rest until the better way is everywhere adopted. This duty the old psychology did not realize: indeed, by its method and results it was cut off from the realization of it. It shall now be my aim to show how contemporary psychology is addressing herself to all these undertakings.

A. Psychology as Research Discipline.—I begin with this point because it is the most striking fact about the present state of psychology in all countries where the experimental idea has been given entertainment. Probably students and general readers hear more about 'research' in connection with psychology than with any other branch. And it is odd—indeed to workers in other departments amusing—that all this claim to research ability, and talk about 'original contributions to knowledge,' is by professors who are yet smooth-faced and generally quite inexperienced in university affairs. A physicist who makes contributions to knowledge is extremely rare, but the 'new psychology' has two men of research to every competent college instructor in its ranks.

This, I take it, is a hopeful and encouraging state of things, and has its origin in two influences: first, the new impulse has come from Germany, where the university-function corresponds very nearly to the graduate-discipline function in the few American institutions where graduate work is encouraged; and second, because the actual state of the subject is such that research is a matter of comparatively less difficulty than in the older scientific branches. Yet the actual value of this condition of things in the permanent development of the subject must be held to be disciplinary and educational; for the more serious and philosophical of the psychologists do not expect these first results of the new methods to be revolutionary in their value, nor have the researches so far published been much more than suggestions of what may be done when the method is held under better control and those who apply it have had adequate discipline and training in its use.

Accordingly, in my view, the very marked tendency to 'research' evident in the management of the new laboratory foundations of the colleges in this country is of main value as offering training to the future instructors in psychology

throughout the land, rather than as offering contributions to knowledge. The students in these laboratories come largely from colleges where experimental psychology is unprovided for or held up for criticism by professors of philosophy. The utilization of their results, except in problems whose solution properly involves ignorance, crudity, and liability to individual variation, is manifestly impossible.

The research discipline offered by graduate work is indispensable, however, as discipline, since it is at present the only substitute for undergraduate discipline. This, indeed, is the function of graduate work in the other departments of science in the universities. It is emphasized, however, in psychology since, as I shall show below, undergraduate instruction in experimental psychology is still in an inchoate condition even in the few larger institutions in which it has been added to the B.A. course of study.

Chairs in Experimental Psychology occupied by men whose principal function is graduate discipline—although in some institutions the undergraduate function is being recognized—are now no longer novelties. Abroad the German universities take the lead in such instruction; yet the instructors are generally professors of philosophy or of psychology who offer experimental courses. Laboratory foundations began in Germany in 1878 with the Institute at Leipzig (Professor Wundt); they are now to be found as well at Berlin (Professor Ebbinghaus, now at Breslau), Göttingen (Professor Müller), Bonn (Professor Martius), Prague (Professor Hering); Munich (Professor Stumpf, now at Berlin), and Heidelberg (Professor Kräpelin). As for other European countries, a chair of Experimental Psychology was founded at Paris in the Collège de France in 1886 (Professor Ribot), and a 'Laboratoire de Psychologie physiologique' opened in the Sorbonne in connection with the École des Hautes-Études in 1891 (Professors Beaunis and Binet). Othersuch Continental foundations are to be found at Geneva (Professor Flournoy) and at Rome (Professor Sergi). At Florence a laboratory and museum of Psychology and Criminal Anthropology has recently been instituted (Professor Mantagazza). In Great Britain and her possessions the analytic method has not given way to the experimental. In Canada

alone, at the University of Toronto (Professor Baldwin, now Dr. Kirschmann), a well-equipped laboratory was opened in 1891, although a little later a small sum was secured for the purpose of beginning work of this kind at the University of Cambridge, England." Lectures are given, however, both by physiologists (Professor Hill at University College, London, 1894) and by professed psychologists (Professor Alexander, Owens College, Manchester, 1893). Japan follows with one such laboratory—that at the University of Tokio (Professor Motora).

In the United States the extension of this method of treatment has been rapid, and the establishment of chairs and of laboratories extraordinary. The first laboratory was established in 1883 at Johns Hopkins University (Professor Hall), but it has since been closed. This was followed in 1888 by the establishment at the University of Pennsylvania of the first chair of Psychology alone with a laboratory (Professor Cattell). Here the first undergraduate laboratory instruction was given. Later, chairs for Experimental Psychology alone have been erected at Columbia College (Professor Cattell), Harvard University (Professor Münsterberg), where an additional Professorship in General Psychology exists side by side with it (Professor James), the College of New Jersey at Princeton (Professor Baldwin). Professorships either in Psychology as a whole, or as associated with Education, exist at Clark University (Professors Hall and Sanford), Wisconsin University (Professor Jastrow), Cornell (Professor Titchener), Chicago (Professor Strong), Indiana (Professor Bryan), Illinois (Professor Krohn), Stanford (Professor Angell), Catholic University at Washington (Professor Pace), Wellesley College (Miss Professor Calkins). At all these institutions laboratories with equipment have been provided; and such provision has been made in others where no separate professorships have yet been erected, i.e., Yale (Professor Ladd), Brown (Professor Delabarre), Minnesota (Professor Hough), Nebraska (Professor Wolfe), Michigan (Professor Dewey, now of Chicago).

The nature of these laboratories is illustrated by the large exhibit already spoken of. That at Harvard University is the largest, best equipped, and most freely patronized by graduate

students. A Harvard pamphlet-catalogue of the apparatus in the laboratory, containing also illustrations, bibliographies, and a list of topics under investigation (23 in number), was prepared by Professor Münsterberg for the collective university exhibit. The rooms given to this science, however, in the universities are usually inadequate and ill-adapted. The only such laboratory yet planned and constructed especially with regard to the requirements of this work is that at the university of Toronto, of which a description with plan is to be found in 'Science,' XIX, 1892, p. 143. The most extensive accommodation provided for this work in America is probably that at Yale, where a house with fifteen rooms is devoted to it. A description of the Yale laboratory is also to be seen in 'Science,' XIX, 1892, p. 324.

The following selected topics set recently for original investigation in two of the institutions may be taken as typical of the kind of themes through which the graduate discipline acquired in all these foundations is secured.

COLUMBIA (1893-4): "After-images—their duration and nature as a function of the time, intensity, and area of stimulation."

"The time of perception as a measure of differences in intensity, and the correlations of time, intensity, and area."

"The perception and attention of school-children."

PRINCETON (1893-4): "The progressive fading of memory for size of visual figures."

"Investigation of memory-types by means of reaction-times."

"Size and color contrast effects on the retina."

"Complex illusions of rotation." *

The treatment of general psychology is adequate as never before, also, in the graduate instruction of the country. The courses of lectures and the instruction by the Seminar method gather large numbers of students who have already graduated in less pretentious colleges. The publication in recent years of so many systematic treatises, especially in America, has

* Similar topics of research at Harvard are to be found (23 in number) in the Catalogue of the Harvard Laboratory already mentioned, and those at Yale in the "Studies from the Yale Laboratory," 1893.

contributed to this; a dominating influence in this matter being a work which has proved to be a *vade mecum* to psychological inquirers—the ‘Principles of Psychology’ of Professor Wm. James.

B. Psychology as Undergraduate Discipline.—The position of psychology in the undergraduate curricula of the leading institutions also invites remark. Two important changes may be discerned in recent years, both indicating the permanent breaking away of this discipline from its earlier hampering connections: first, the recognition of the aim of the science as self-knowledge and self-control; and second, the introduction of the experimental method of instruction.

The first of these tendencies is shown in the remarkable change worked (and still working) in the qualifications and training of the occupants of chairs in Philosophy and Psychology. Even the smaller denominational institutions are following the lead of the great eastern foundations, and of the progressive state universities, in seeking men who are trained to the same rigorous interpretation of fact and search for it that are the first requisites of the genuine *Naturforscher* in other branches of science. The guardianship of this important realm, the mind, from outside, in the supposed but mistaken interests of religious and ethical truth, has had its day in many institutions—at least in any sense that denies to the investigator and teacher the full liberty of disputing hypotheses which facts do not support, and of stating those, however novel, which well-observed facts do support. Consequently Philosophy and Psychology are now self-controlling departments in the colleges; and so the courses of psychology are arranged with view both to the adequate instruction of the student in its history and results, and with view to that high discipline which the pursuit of the ‘moral’—as opposed to ‘physical’ and ‘natural’—sciences undoubtedly gives.

Second, the introduction of the experimental method of instruction has had its beginning. It consists in the actual demonstration of the leading facts of Experimental and Physiological Psychology in the class-room with added opportunities for students to perform them upon one another, and, under certain topics, upon the dissected nervous systems of

animals. One of the results is the greater concreteness and interest given to the subject for younger students and the correspondingly increased election of all the branches of the tree of philosophy in the later years. The union of the two functions of introspection and experimental observation thus secured renders this branch, in my opinion, of unique and as yet undeveloped value in the total discipline of college life.

It is evident that this undergraduate service cannot be adequately realized until the science which aims to render it is itself well developed and sufficiently categorized. The actual condition of things suggests encouragement, therefore, but not enthusiasm. It is evident that such a method of instruction is at present impossible to any but the original workers in this field, and they indeed are each a law unto himself. There are very few experiments of a psycho-physical or psychological kind which are of such evident importance and value as to be recognized by all as available for class demonstration. And a more radical defect is that there are very few principles as yet formulated which can be adequately demonstrated by single or grouped experiments. Add to this the fact that the whole exhibit of apparatus at Chicago contained very few things which are suitable and convenient for untrained use or illustration, and the difficulties become in part apparent. It is a duty which experimental psychology owes to education to meet this need by bringing her results into line with the more elementary principles of general psychology, of providing simple apparatus which can be used by less expert instructors, and of preparing text-books for junior classes. While no text-book to-day exists for this purpose, it is yet gratifying that two such 'Courses in Experimental Psychology' have already been announced by competent writers, both Americans (Professor Cattell of Columbia College and Professor Sanford of Clark University).

Reference to the latest catalogues of Brown, Wisconsin, and Michigan Universities (not to mention many others) may serve to show the nature of the courses offered in institutions where the work is as yet mainly undergraduate.

C. Psychology in its Bearings on Pedagogy.—Finally, the relation of psychology to the science of education may be given

a word after the discussion of its place in practical education. Pedagogy as a science treats of the application of psychological principles to the development of normal and cultured personality. The ground-work of such a science must be afforded therefore by psychology: and inasmuch as the teacher has to do with body as well as mind and with mind principally through the body, it is experimental or psycho-physical psychology to which this duty to theoretical education mainly comes home. It is needless to say that there is no such science of pedagogy in existence. Most of the books which have heretofore appeared in America on this topic—and their name is legion—are unworthy of serious attention. Further, the importation of the German *a priori* 'Systems of Pedagogics' finds its main service in keeping awake the expectation and the *amour propre* of teachers: not in affording them much empirical assistance in their task. Yet it is encouraging that the phrases 'child-study,' 'self-activity,' 'apperception,' 'scientific methodology,' are in the air, in this year of the Exposition, and every teachers' convention listens to hours of paper on such topics.

Contemporary psychology is becoming aware of this duty also, however far she may yet be from performing it. Children are being studied with some soberness and exactness of method. Statistical investigations of the growth of school-children, of the causes and remedies of fatigue in school periods, of the natural methods of writing, reading, and memorizing, are being carried out. The results of several such inquiries were plotted for exhibit in the department of Anthropology at Chicago. Questions of school hygiene are now for the first time intelligently discussed. The relative values of different study-disciplines are being weighed in view of the needs of pupils of varying temperaments and preferences. And it only remains for the psychologists—themselves teachers—to set the problems and establish the methods, and all the enthusiasm that is now undirected or misdirected will be turned to helpful account. Among those who have addressed themselves to this task in this country with information and influence two names may be mentioned, that of W. T. Harris, U. S. Commissioner of Education, Editor

of the 'International Education Series,' which now includes 24 volumes, and President G. Stanley Hall of Clark University, Editor of the 'Pedagogical Seminary' (vol. I-III, 1891-4). Another journal which is doing good work for sound education is the 'Educational Review,' edited by Professor N. M. Butler of Columbia College (vols. I-VII, 1891-4).

V. PSYCHOLOGY AND OTHER DISCIPLINES.

It is necessary, in conclusion, in order that this report may adequately present the conditions under which psychology exhibits herself and her historical progress, to speak briefly of the relations which this topic sustains to the other 'moral' forces which make up largely the culture element in our present-day social environment. The traditional connection with philosophy is not severed by the new directions of our effort, but on the contrary they are made more close and reasonable. The change in psychological method was due in part, as I have said above, to changes in philosophical conception; and it is only part of the same fact that scientific psychology is reacting upon philosophy in the way of healthful stimulus. Both the critical idealistic and the critical realistic methods of philosophy are richer and more profound by reason of the lessons of the new psychology. It was only just that the science which owed one of its earliest impulses in this country to a book from an advanced thinker of the former school, the 'Psychology' of Professor John Dewey of the University of Michigan, should repay the debt by its reconstruction of the Kantian doctrine of apperception in terms acceptable to the later thinkers of that school. And it is no small gain to both schools that their issue should be joined, as it is to-day, on ground which stretches beyond their old battle-fields by all the reach of territory covered by the modern doctrines of Naturalistic Evolution, and the Association Psychology. Philosophy escapes the charge of Lewes that her discussions are logomachy, when the disputants on both sides are able to look back upon those even of the late period of Lewes and admit the essential truth of both of their hotly-contested formulas. As far as this is the case, I venture to say that it is due to the

progress of psychology in giving content to the terms of the logomachy and in enabling the best men to reach more synthetic and more profound intuitions.

The relation of psychology to theology also is now as close as ever, and must remain so. And the obligation must become one of greater mutual advantage as psychology grows to adult stature and attains her social self-consciousness in the organization of knowledge. The benefits which theology might have gained from psychology have been denied in great measure through the unfortunate attempt to impose the theological method upon the treatment of the whole range of mental fact. The treatment of 'Anthropology' included in the text-books of systematic theology bears about the same relation to that of current Psychologies like Höffding's and James' as the physiology of the philosophers not long since bore to the work of the neurologists and morphologists. It is evident, however, that this condition of things is now happily mending; and it is to the credit of one man, ex-President James McCosh of Princeton College, that he first of the theologians who were teaching philosophy in this country welcomed and advocated the two new influences which I have taken occasion above to signalize as the causes of the better state of things: the influence of the German work in psychology (Preface to Ribot's 'German Psychology of To-day,' 1876) and that of the evolution theory in biology ('Religious Aspect of Evolution,' 1888).

Finally, I may note the growth of a new department of psychological study which aims to investigate the mental and moral life of man in its social and collective conditions. The evident need in such subjects as Sociology and Criminology is the knowledge of the laws of human feeling and action when man is found in crowds, orderly or disorderly, and in organizations, legitimate or criminal. This need is now beginning to be felt both by sociologists and by psychologists, and we may hope that the questions already started in Italy by Ferri, Sighele ('La foule criminelle,' 1893), in France by Tarde ('Les Lois de l'Imitation') and Guyau ('Education and Heredity,' Eng. trans. 1892), and in England by Spencer, may receive fruitful development in this country. It is an interest-

ing sign of the times in education that the theological schools are beginning to realize the need of such knowledge of collective man, as part of the training of the ministry. Instruction in social questions is made a separate department in the Yale Divinity School and in the Chicago Theological Seminary, as well as in other such institutions.

En résumé, I have only to add that psychology is now the branch of knowledge which is developing in most varied and legitimate ways; and that the exhibition made at the Columbian Exposition, while not adequate in many respects, yet served, to those who studied it intelligently, to indicate the present gains and the future prospects of the science.

17

18

19

20

21

22

23

24

25

26

27

28

29

30

31

32

33

34

35

36

37

38

39

40

41

42

43

44

45

46

47

48

49

50

51

52

53

54

55

II. FREEDOM AND PSYCHO-GENESIS.

BY PROFESSOR ALEXANDER T. ORMOND,

Princeton University.

There is a tendency in the thinking of the time to evade the question of the freedom of the will. Some excuse themselves for this neglect on the plea that the issue has become antiquated or exploded. But so long as the sense of responsibility for his actions survives in man, the question of freedom will remain central for him and his interest in its solution will be vital. We may assume then that neither the psychologist nor the metaphysician can waive the responsibility of its consideration.

Much of the perplexity that surrounds the question arises from the absence of any definite concept of the nature of the subject under debate. Usually there is in the minds of both the asserters and deniers of freedom a kind of vague apprehension that it is somehow inconsistent with the idea of law, and that a world of freedom would be virtually the same thing as a world of chance. To a mental state like this the alternatives are chance and fate, and the only escape from the iron clutches of an all-devouring necessity seems to be through a repeal of the law of causation and a plunge into 'primal eldest chaos.'

The dilemma which thus arises supplies a problem to the psychologist, although the source of the difficulty is partly extra-psychological and consists in the assumption that mechanical law or determination by other is the only conceivable type of orderly activity, and that it must be extended over human volition, unless we are prepared to regard the will

as lawless. The resources of psychology in dealing with the question are both direct and indirect. The direct method of approach is through the analysis of the activity of choice as it manifests itself in consciousness. If we separate this analytic business from all questions of the remote antecedents of choice and set ourselves to obtain as adequate an intuition as possible of the actual factors which enter into a present act of volition, we shall, I think, reach something like the following conclusion. In the first place the idea of motiveless choice, for us men, must be dismissed to the limbo of exploded philosophical myths. Motivation may be assumed as a universal law of choice, and the initial question will be to determine the mode of the operation of this law. Here we take the first step that lifts the issue above the plane of both fate and chance. Psychological analysis proves the immanent character of all normal motivation. Whatever relation the remote grounds of our actions may bear to us, the immediate determinants of choice and action must, in order to influence the will, become internal as parts of the energy that wills and chooses. Determination here is not external but internal. This conclusion taken in connection with two additional considerations will suffice to give a fairly adequate notion of the nature of the voluntary function.

One of these is the selective character of choice. True choice is always a case where one is taken and another left. There are, it is true, influential psychologists like James who regard ideomotor action, that is, immediate reaction upon presentation, as the type of all volition. Against this position the objection holds, I think, that it reduces all choice to immediacy and leaves no place for deliberation. But the choice that we mortals know most about is a mediate function which operates through selection of alternatives. And selection of alternatives involves a two-sided process, conscious annulment of ends as well as conscious self-commitment to the end that is chosen. The remaining feature of choice that is vital to it is the power of arrest which the mind is able, through its command of attention, to exercise over the forces that are impelling it to volition. Through this power of arrest the mind is able to effect a stay of the voluntary proceedings until it has

collected its scattered forces and is in a position to act as a unit. Thus, in what we may call normal choice the determining motive is the whole self that chooses,* while abnormal forms of choice would arise as departures and aberrations in various ways from this normal standard.

This is perhaps as far as the direct analysis of consciousness can take us in determining the nature of choice, but it is far enough to justify several important conclusions. The first of these is that the activity of will cannot be subsumed under the category of mechanical causation whose form is determination by other, but that in will we come upon a form of activity that is self-determining. We have seen that the immediate antecedent of choice, when it is normal, is the whole present self. In choice then the mind simply determines itself from one state to another. If we represent the two states by a and b and the activity of choice by x , every case of normal choice will involve the self-moving of the mind from a to b through function x . The causal antecedent of x is, therefore, the mind in state a , while the consequent is the mind in state b , and x is the activity or movement in which the transition is made. Normal choice is, therefore, self-movement and not movement by other. Another conclusion that follows from the above analysis is that fatalism rests on a false idea of the relation of a man to his own choice. The fatalist is one who denies his own agency in volition. The only type of determination, in his view, is determination by other. He, therefore, makes a false diremption between himself and the determining causes of his action and conceives himself to be a mere puppet in the hands of God, Nature, Fate, or whatever his Absolute may chance to be. But if the immediate antecedent of choice is the chooser himself, and if choice is self-determination, the presupposition of fatalism falls to the ground; for, however a man's choice may be determined, it cannot be that he is a mere spectator of the drama, or that he is run by alien forces that act without his own assent.

Self-determination is freedom: or, if we regard it as a type

* Two interesting discussions of the relation of motive to choice are Baldwin's—*Hand-book of Psychology*, Vol. II.: *Feeling and Will*, pp. 352-376; and Hodgson's—*Mind*, April 1891: *Free Will: an Analysis*.

of causation, it is free causation. That freedom is realized, therefore, in the form of volition is a psychologically verifiable fact. But in arguing the question we have distinguished the present act of will from its indirect antecedents and conditions. They are, however, never separate in fact, but the present choice is, in some sense, what it is, because of its antecedents. This changes the issue into a question of predeterminism. It may be demonstrated that the present choice is self-determined, and at the same time the self that chooses may be pre-determined by its antecedents. We may thus escape fatalism and still find ourselves in the clutches of necessity.

It is clear that the issues involved in this phase of the question cannot be settled by an appeal to the individual consciousness. The problem of predeterminism is one that involves the factors of heredity and environment, and the point to be debated here is the relation of the present self that chooses to these predetermining agencies. At the basis of the inquiry rests the fact of a developing series the parts of which are bound together by the law of causation and all of which are, therefore, dependent on the chain in which they constitute individual links.

Now, the series with which the psychic nature of man is most completely identified is the biological. Man is a living being and his psychic activity is a species of life. This does not, however, reduce psychology to a branch of biology, but rather comprehends the biologic activity in that of the soul, just as the intelligence of the animal is comprehended in that of man. The term that is central in the biological series is the germ-cell out of which the organism develops and through which it propagates its species, and it is in connection with it that the bearings of heredity and environment need to be primarily estimated. Of the two factors, that of heredity is clearly the more fundamental, since it is through its agency that each successive environment is supplied with the special material upon which its modifying forces are to play.

How then are we to conceive heredity? It is clear that the germ-cell is the medium through which persistent effects must be produced. But at the very threshold of the inquiry into the nature of heredity, biologists have split into two con-

tending camps known as Neo-Lamarckians and Neo-Darwinians, the leader of the latter school being Professor Weismann, whose whole doctrine of heredity rests on the assumption, which is beyond proof, that the germ-cell out of which the organism develops, after it has separated from the parent organism and become fertilized, breaks into two parts, one of these developing into the new organism which is open to the modifying influences of the environment, while the other part remains unchanged as the germ of a future organism. Weismann, therefore, denies the modifiability of that part of the germ-cell through which the continuity of the species is maintained, and on this ground denies the transmissibility of acquired characters or modifications. Having virtually eliminated the environment as a factor in development, the Neo-Darwinians have three agents left: (1) new combinations of original characters which are effected through the modes of transmission, sexual or asexual; (2) accidental variations, or the appearance of characters which cannot be accounted for by the first cause; (3) natural selection which tends to eliminate all variations arising through the first two agencies, that are useless or injurious, and causes only those that are positively useful to survive.

Now, a careful analysis of these factors gives us the somewhat startling result that a whole class of variations, those that have no ancestral copies and on which development most directly depends, are left virtually unaccounted for. Darwin himself regarded variations in general as accidental; at least he brought forward no theory of explanation, while the Neo-Darwinians are able to account for some variations by new combinations of ancestral copies, but they have no adequate explanation for that large class of changes which the opposing school of biologists are in the habit of ascribing to the modifying influences of the environment.

It is because the Neo-Lamarckian school have command of all the Weismannian resources and are able in addition to fall back on the modifying activity of the environment as a cause of original variations, that their doctrine seems to possess a decided advantage over that of their rivals both as a theory of development and of heredity. They reject Weismann's

absolute isolation of the germ-cell of future organisms and hold that it is to some degree open to the modifying influences that affect the present organism in which it dwells. They are thus able to reach an idea of the development of organisms that is more flexible than the Weismannian, since the germ-cell is represented as fluent and open to all sorts of modifying influences; as well as more completely mechanical, inasmuch as the results are represented as arising out of a long series of almost infinitesimal changes produced by the varying play of environing forces.

The functions of heredity and environment will be most adequately conceived when considered in their relation to the germ-cells out of which the successive organisms develop. We have in the germ-cell a biological unit which contains the stored-up potency of a developed life, there being included in this unit as part of its potential, the accumulated modifications of a series of antecedent environments. And this unit containing the results of past modifications is to be conceived as continuing susceptible to all the modifying influences that affect the parent organism in which it is latent as well as to the more effective agencies which play upon it after it has become the active germ of a new organism.

The history of the living organism may be taken as including that of the mind; for whether we regard the mental as involved in the original potency of the germ-cell, or as super-induced upon it at some stage of its development, in either case its fortunes will be cast in with the biological unit with which it is associated and through this connection it will be vitally affected by all those hereditary and environing conditions which influence the organism. Professor Orr in his work entitled *A Theory of Development and Heredity* has made a very interesting contribution to the psychology of the hereditary and environing forces. His contention is that the nervous system stands as a necessary medium between the environment and the living organism, translating the forces of the former into nervous energy, in which form it becomes the working agent in every part of the system. Now, the nervous force builds the organism, especially on its functional side, by means of two psychological laws; namely, repetition and asso-

ciation, and Professor Orr shows in several chapters of his book how in the sphere of psychic activity the operation of these laws leads to the development of habitual responses to the forces of the environment and how these tend to become ingrained in the nervous tissue and to be transmitted by heredity as the organized physical basis of instinct and mental habits.

The logical import of such considerations as these seems on first sight to be the suppression of freedom and the re-instatement of strict mechanical necessity, and this is the conclusion drawn by physiological psychologists like Dr. Maudsley and Professor Ziehen, who dismiss freedom as pure illusion, asserting the connection between choice and its antecedents to be essentially the same as that between a physical cause and its effect. It would be useless to deny that from the common point of view these conclusions are not without some reasonable grounds. If the will of man is strictly predetermined by its antecedents; if its choices are but links in a chain of mechanical causation, it would seem that the fact that the form of choice is self-determination loses most of its value, and I am unable to see how a libertarian could continue to fight for it with much stoutness of heart. But the irony of the situation arises here in the fact that at this point the investigation is usually dropped and the inquirer goes his way thinking he has solved the problem. As a matter of fact he has only succeeded in stating some of its data and the solution is yet to be achieved. In the preceding investigations we have simply been getting at the two sides of our problem. We have demonstrated two conclusions. The first is that all choice is self-determination; that normal choice is the unimpeded and full expression of the individuality of the chooser. Nothing that we have discovered since has overthrown that conclusion. It still holds that man himself chooses and that his choice is not a function of some external necessity. The second conclusion demonstrated is that this self that chooses belongs to a mechanical series and has been helped to its present position by the forces of heredity and environment. Choice is self-determined, but the chooser is predetermined through heredity and environment.

We have to deal then with the two factors, mechanism and self-determination. Any freedom that is open to man must include both. It is clear that if freedom and mechanical causation are mutually exclusive terms, freedom for man is a chimera. Mechanism cannot be expelled from his activity, but is inseparable from its highest equally with its lowest phases. The freedom that is open to man must be one that can be realized through and in connection with mechanism. Is any such freedom possible? In seeking an answer it is to be noted in the first place, that the problem of freedom in this larger sense could only arise to a consciousness that had stumbled upon a dualism and had been brought face to face with the alternatives of a higher and a lower self. When the actual consciously faces the ideal whose claim to legislate for it by imposing upon it a law of duty, it recognizes, the question will inevitably arise as to the practicability of obeying the law of the ideal and realizing the higher life which it enjoins. This was the issue as it presented itself to Kant, and in his attempt to solve it he committed what seems to me to be his gravest theoretic mistake. Kant proceeds on the assumption that the ideas of mechanical causation and freedom are mutually exclusive and that the same system of reality cannot contain both, and he thinks, therefore, that in order to establish the reality of freedom it will be necessary to show that outside of the bounds of mechanism there is a sphere of psychic activity that is unaffected by mechanical conditions. The only conclusion Kant could reach from such grounds was the one he actually drew; namely, that while there may possibly be a transcendent region in which such activity is conceivable, yet so far as actual experience goes we never get beyond the reach of mechanical influences.

This conclusion is instructive not only as to Kant's state of mind, but also as revealing the morass in which so many contemporary thinkers are still floundering. Kant's trouble arose from the fact that while he had a very keen intuition of the mechanical conditions with which the mental life is begirt, he had scarcely any notion at all of psycho-genesis. Otherwise, those forces which seemed to him only to bind and circumscribe would have appeared in a new light as conditions of develop-

ment. As it was, Kant could only sit and wring his hands and wish that the universe were different from what it is, until in a happy moment it was borne in upon him that the difficulty might be overcome by tagging freedom on to the end of a moral postulate. But this, at the best, turns out to be a sort of device by which morality may comfort itself, the actuality being different. It is not open to the contemporary thinker who has become disillusioned on this point to betake himself to the Kantian refuge, and it has not occurred to him, as yet, to apply the genetic idea to the question of the relation between mechanism and freedom.

The most pregnant application of the genetic idea to the basal problems of psychology that has ever been made is that of Aristotle. It arises through his translation of the ontologic ideas of Platonism into the formal principles of individual things, and his conception of these forms dynamically, as activities which tend to unfold from a mechanical state of mere potency or capacity toward one of actuality or a state of self-activity. This view is involved in his treatment of the three categories, *Δύναμις*, *Ἐνέργεια*, and *Ἐντελέχεια*. *Ἐνέργεια* is the category of self-activity in its absolute form, while *Δύναμις* and *Ἐντελέχεια* stand as a pair of correlatives which together embrace nature and relativity. They also represent the opposite poles of a process in which nature is conceived as passing from a stage of matter, or pure mechanical response to external impulsion, to that of soul, in which mechanism is subordinated to the form of self-activity. Soul, in Aristotle's view, is the climax of nature and embraces in its constitution a synthesis of passivity and actuality. This appears in his definition of it as the 'first Entelechy' of a body that has the capacity of life. The fine point of the definition is apprehended only when the dual significance which Aristotle attaches to the term *Ἐντελέχεια* is kept in mind. This term, as he uses it, is a sort of watershed between potency and actuality, giving a reminiscent look toward mechanism as well as a prospective glance toward the self-activity of spirit. Soul, then, as the first entelechy of a potentially living organism, is to be conceived at any and every point of its life as embracing a synthesis of polar moments, passivity

and activity, potence and actuality, and this synthesis may be regarded as grounding the relations which arise later between the categories of mechanism and spirit, determination by other, and free self-activity. But this is anticipating. Again, Aristotle's definition connects soul with life as a form of its actualization. The highest form of life is soul. This is Aristotle's doctrine. It escapes the dualism of the theory that soul is a distinct principle introduced into the living organism, and plants itself firmly on the ground that life is one, that it is not completely actualized, and that it does not reveal its true and complete nature, anywhere else than in soul. But the point of vitalest interest in connection with the special theme of this paper is the fact that Aristotle's conception of soul and its relation to life enables him to incorporate the principle of development into its very constitution in such a way that it can no longer be adequately represented under static categories. And it is here that the Aristotelian conception of the soul seems to me to furnish a much more adequate and effective basis for psychology than that of Herbart-Lotze, for example, in that it shows more clearly how the genetic method may be grounded in a real principle of psycho-genesis.

I mean by a real principle of psycho-genesis one that not only grounds development as a constitutional law of the psychic life, but also supplies some definite notion of what *psychic* development means. The Aristotelian concept helps to the formation of such a notion in this way. It asserts, not simply that soul-life is a development, but that it is a development of a particular species; namely, of a principle of self-active consciousness, from a state of potence or mere capacity up to a state where all its powers shall have become actual and its nature completely revealed. The nature of the psychic principle and the species of its development are thus to be determined in view of their outcome. If the actualized result is a self-active and self-determining consciousness, then we have the right to say, on the Aristotelian principle, that it was potentially that from the start, and that in every stage of its evolution it was going on to be just that. And without raising any question of transcendent teleology or design, we see how the process is immanently teleological from the beginning.

The value of the Aristotelian insight will be manifest in view of the fact that the two most pregnant ideas in the domain of psychology to-day are these of psycho-genesis and the immanent teleologic character of consciousness. The tendency of the one is to modify static conceptions and to view the soul-life as fluent and progressive; that of the other is to shatter the hard front of mechanism and to reduce it to the position of a servant to a teleological process. The Aristotelian insight enables us to ground these categories in the very constitution of the soul itself. So that when we find consciousness to be a selective principle which is everlastingly in pursuit of ends even when it does not know itself to be teleological, we can rationally ground the discovery in a doctrine of the nature of the soul as a self-active principle whose law is development from mere potency into the actuality of a self-conscious and self-determined life.* And when we find in consciousness a dualistic dialectic between an empirical will and an ideal which utters itself in conscience, we are able to trace this dialectic to the teleological law of psychic development, which is the law of the immanent ideal activity that the psychic process is ever going on to actualize.†

We conclude then that all psychic activity is in its essential nature teleological. What it actually is or realizes, never truly or completely expresses its nature. But its real character only comes out in the light of what it has in it to become, or what it is going on to be. Now in the light of this we ask why freedom should not be teleologically construed. In the former sections of this paper we demonstrated two conclusions; namely, that normal choice is a form of self-determining activity, and that in its connection with heredity and the environment, the self that chooses belongs to a causal series and is predetermined. In view of current modes of thinking the last conclusion seemed to swallow up the first and to leave the life of man in the clutches of necessity. But when

* The Aristotelian idea of soul thus seems to supply a rational basis for James's doctrine of the selective character of consciousness.

† I do not mean to assert that conscience is completely explained as the immanent ideal of the soul. In my work on 'Basal Concepts in Philosophy' I seek to show the relation of immanence to the transcendent. The point here is that conscience on its psychic side utters the immanent ethical end of the soul.

in the light of later conclusions we claim the right to put a teleological construction on the whole process, the clutch of necessity seems to be loosened. For the developing series then acquires a meaning outside of the mere determination of consequents by antecedents. Instead of a soulless corporation, it becomes animated with spirit, and we see that what has outwardly the appearance of dead mechanism becomes a fluent and living organism whose whole significance is the immanent potency which it contains and the immanent end or ideal which it is going on to realize.

It is clear that from the teleological point of view, whose justification has been shown to spring from a profound view of the nature of psycho-genesis, mechanism becomes the hand-maid of teleology, and while it conditions, also furthers the immanent end. Heredity conserves the end by preserving and transmitting the gains of individual experiences, while the environing forces supply the necessary stimuli of development. And when we apply these considerations to the problem of freedom it becomes clear that the moment we subordinate mechanism in general to teleology, we thereby subordinate mechanism also to freedom. And instead of standing by and wringing our hands because predeterminism swallows up freedom we may go on our way rejoicing, since our new insight enables us to see that nothing of the sort happens, but that free self-determination is the end which all this hard and forbidding-looking mechanism has had at heart and has been realizing from the beginning. For, just as the end subordinates the means, so freedom subordinates the mechanical agencies through which it is achieved.

There is no reason why psychology when it has committed itself to the genetic idea should stubbornly persist in construing freedom in some absolute sense which is above man and then deny its existence because it is inconsistent with the mechanical conditions of human life. Why should not freedom be construed in harmony with development, and why should it not be teleologically conceived? The questions supply their own answer. The teleological idea of freedom is the only one that a genetic psychology can consistently entertain. For, to genetic psychology conscious activity *is* teleologic activity, and volition is the type of conscious activ-

ity on the practical side. Volition is self-determining activity, as we have seen, and self-determining activity is free activity. If free activity is the outcome of mental development and this outcome is the immanent end and meaning of the process, the conclusion naturally follows that the development only achieves its complete reality in freedom.

Now, if we identify freedom with self-activity and construe it teleologically, there are several senses in which the term may be used in its relation to mental development. As potency or capacity for self-activity, it will be a condition of development. As actual self-determination it will be the form of all normal choice; whereas, as the self-determination of the ideal it will be the end toward which development is tending but which it never realizes. But in each and all of these senses its vital relation to experience is evident. Freedom is not a speculative will-o'-the-wisp, but it is something that, in the words of Bacon, concerns 'men's business and their bosoms' in that the possession of it is the condition of their being men, while the realization of it is the great end of rational and spiritual activity.

The doctrine of freedom here developed has also another merit. It supplies a rational ground of distinction between the normal and the abnormal in the sphere of choice. Freedom can be postulated without qualification, only of normal choice. The normal function of heredity and environment is the development of free activity. In other words, the normal is the good. The abnormal will enter as some kind of evil or aberration from the normal standard, and while it will be negative, it will be also real. The abnormal will become a factor in both heredity and the environment, and it will operate as a kind of loading of the dice, and in the development of predispositions to evil, in diminishing and thwarting and turning aside the forces of development. The abnormal will embody itself in organic and functional defects, in ingrained hereditary evil tendencies, in environments which hinder and clog progress. The abnormal thus supplies a special problem to the psychologist as it does also to the moralist and the jurist. But to the psychologist as well as to the moralist and the jurist a correct diagnosis of the normal is a necessary condition of the rational treatment of the abnormal.



STUDIES FROM THE PRINCETON LABORATORY (I-V.)¹

I. MEMORY FOR SQUARE SIZE.

BY J. MARK BALDWIN AND W. J. SHAW.

The experiments of this study were performed at Toronto by Prof. Baldwin and Mr. W. J. Shaw, during the winter of 1892-3.² The object was to determine the accuracy of the memory for *size*, as affected by the lapse of time. A figure of two dimensions was selected for experiment because of the tendency to measure linear size in terms of well-known units of length. Circles tend to be measured by their radii, but in the case of the square, the impression is that of the area, and the natural memory-image is not so liable to be corrected by comparison with standards fixed in mind by repeated experience.

The experiments proceeded by three different methods: (1) *Selection from a Variety*. A single figure (the *normal*, 150 mm square) was drawn on a black-board and shown to a large college class; after a certain time a number of squares of various sizes were shown simultaneously, and the class was requested to designate the one that appeared to be the same size as the normal. The squares ranged from 130 to 210 mm by intervals of 20 mm, and the time intervals were 10, 20 and 40 minutes. The class consisted of about 225 persons, of whom some 50 were ladies. (2) *Identification*. Here the normal square was first shown, and afterwards one other square; the subjects were asked to say whether the latter appeared to be greater, equal to, or less than the normal. The time intervals were the same as before, and

¹ These studies were all concluded in the college year '93-'94.

² Reported in these words to the Amer. Psych. Ass., Dec., 1893, by H. C. Warren.

the second square was in every instance 20 mm greater than the normal.

Both series were treated by the Method of Right and Wrong Cases, and the two results showed remarkable agreement. The percentage of right cases is shown in Table I.

TABLE I.

	I. By Selection.	II. By Identification.
10 min. . .	64.1	87.6
20 " . .	59.3	82.7
40 " . .	36.4	58.5

Plotting the results (Fig. 1), we find the *memory curves*, as they may be called, practically parallel, but the degree of accuracy is much higher by the second method than by the first. In each there is a rapid falling off at first, then a period of gradual descent, and finally another rapid drop.

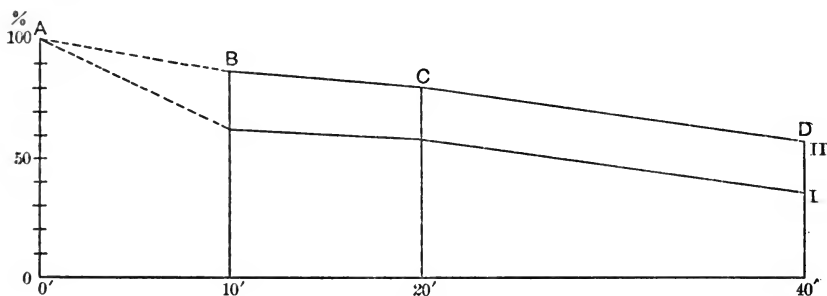


Fig. 1

The greater accuracy of the results in II is partly due to the manner of stating the question. Should the memory-image of the normal square either remain unaltered, or decrease in size, the subject would respond correctly that the second square was the greater, and he would respond incorrectly only if his memory-image had *increased* sensibly in size from its original. Whereas, in the series by Selection his responses would be classed as incorrect if his memory-

image had *either increased or decreased* sensibly. A further source of error in the series by Selection was the disturbance due to simultaneous contrast between the figures. Some special experiments were afterwards made to determine the effect of this contrast (see Study II, below).

In discussing the form of the two memory-curves so reached, it must first be observed that their real origin is not at A, but at a point, or points, near B. For the difference of 20 mm is very much greater than the least discernible difference between two squares observed in immediate succession; hence, even if a considerable interval should elapse before the second square is shown, *no incorrect judgment will be given*. The effect of this is to make the first falling off, when once it begins, even more rapid than indicated on the diagram, and possibly also to carry out the parallelism between the two curves still further. The reason for the sudden falling off may lie in the conditions of the experiments. The subjects began to take notes on a lecture immediately after the normal square was shown, and there was consequently a sudden withdrawal of attention from the memory-image, allowing it to decrease greatly in distinctness at once. After this first influence had taken effect, there was, it seems, but little change until the ordinary factors which tend to make the image more vague, began to take effect. The work of these factors, which one would scarcely expect to become apparent within 40 minutes, may have been hastened by the fatigue arising from steady application.¹

(3) The third series proceeded by what was termed the *Method of Reproduction*. A normal square having been shown, as before, the subjects were asked, after the stated interval, to draw a square of the same size on paper. The normal in this case was 170 mm square. The reproductions were almost always too small, their average being 146.0 after 20 min. and 146.4 after 40 min. This result was rather unexpected, as the other series had indicated a tendency of the memory-image to increase in size beyond the original. It may be attributed to two factors: (1) The muscles of the

¹The results were examined for a possible difference between the two sexes, but the variations were neither marked nor constant in direction.

hand were fatigued from continuous writing, and this tended to give the impression of a figure larger than that actually drawn. (2) The paper on which the drawing was made was not much larger than the actual size of the normal; any figure coming close to the edges would appear very large, since it occupied so large a portion of the field. Hence there was a tendency to draw the square too small. On this account it was decided to separate the results obtained by this method from the others, where the conditions were more nearly alike.

II. FURTHER EXPERIMENTS ON MEMORY FOR SQUARE SIZE.

BY H. C. WARREN AND W. J. SHAW.

The experiments were taken up at this point by Messrs. Warren and Shaw, at Princeton. A possible objection to the Selection Method lay, as has been said, in the disturbing influence of simultaneous contrast. To investigate this, the following experiment was performed: Ten squares, ranging between 100 and 190 mm, were drawn in promiscuous order on a large sheet of paper; on another sheet of the same size a single square was drawn as normal, and the two sheets were placed in different rooms. The subjects observed the normal first, and going at once to the other room designated the square which appeared equal to it. The normal used was 120 mm in one instance and 170 mm in another. In each case there was a marked *attraction towards the center of the series*, the average for the normal of 120 mm being 123.3, and for that of 170 mm, 165.

On this account it seemed desirable to supplement the Toronto experiments by others, and to employ a somewhat different method, using a series which combined the advantages of Selection and Identification. The object was to determine the threshold, *i. e.*, the (average) least perceptible difference from the normal after a given period of time. In each experiment the normal was first shown, and after the interval another square as near the threshold as the latter could be determined from the previous experiments; the experiments were continued until the threshold was found.

When the squares were shown in immediate succession (interval of *no* minutes = perception), the threshold was found to be 3 mm for squares of about 150 mm. When the interval was increased it was found to make an essential difference whether the second square was the larger or the smaller. For an interval of 10 minutes the threshold was 8 mm if the second was smaller, while it was but 5 mm if the second was larger; for 20 minutes it was somewhat less than 8 mm if the second was smaller, and less than zero (a minus quantity!) if the second was larger; that is, when two squares of the same size were shown, 20 minutes apart, *the second was pronounced the smaller* by over 50 per cent. of the subjects (actually, 63 per cent.)

That this result was not accidental (the conditions rendered any collusion impossible) was proved by the substantial agreement of all the experiments, pointing as they did without exception in the same direction. The entire series (marked *a* in Table II) was performed on the same subjects, a college class of about 50, Juniors and Seniors, on nine separate occasions, the 10-minute intervals being taken first. Besides this the table shows two experiments (marked *b*) on

TABLE II.

Interval and order.	Difference between I (normal) and II.						
	20 mm	12 mm	10 mm	8 mm	5 mm	3 mm	0 mm
0 min. II < or > I	—	—	95 (c)	87 (c)	63 (c)	4 mm = 59 2 mm = 44 (c)	85 = (c)
10 min. II < I	—	—	—	—	50 (a)	—	{ — —
10 min. II > I	87 (d)	—	70 (a)	53 (a)	—	—	
20 min. II < I	—	—	75 (b)	94 (a)	75 (a)	65 (a)	{ 63 < 24 = 13 > (a)
20 min. II > I	82 (d)	82 (a)	37 (b)	67 (a)	—	—	

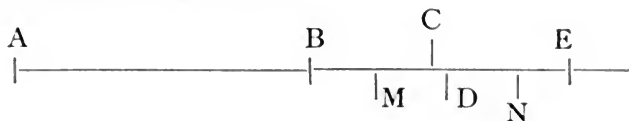
The figures denote percentage of right answers, except under 0 mm, where they denote the judgment (=, >, or <.) actually made. The normal was 150 mm square.

two other college classes of 50 and 65 respectively, where squares of 150 and 160 mm were used, with a 20-minute interval, the normal being smaller in the former case and larger in the latter. The lack of practice makes the threshold much greater in these instances than in the others, but they exhibit a similar difference, depending on the order of sequence. The line of values marked *c* shows the experiments on squares immediately succeeding one another (0 minutes interval), taken with still another set of subjects, and the two values marked *d* are taken from the earlier experiments by Identification.

These results unite to show that besides the growth of inaccuracy, or indistinctness, in the memory-image, there is another factor at work, by which *the memory-image tends to grow larger* as the time interval increases. The table gives three cases which allow direct comparison between an increasing and a decreasing sequence: (1) With unpracticed observers (see *b*), 10 mm *increase* from the normal was noted by only 37% after 20 minutes, while the same amount of *decrease* was noted by 75%. (2) With practiced observers (*a*), 8 mm increase was noted by 67%, and the same decrease by 49%. (3) With the same observers as (2), the final test, after considerable practice, was with two equal squares, separated by 20 minutes interval; 63% pronounced the second square smaller, 24% equal, and 13% larger. Comparing this with the observations on the threshold for perception, we see that while half of the subjects can distinguish a difference in the latter case only when it amounts to 3 mm, in case of a 20-minute interval a majority actually think they perceive a difference *when none exists*, indicating plainly that their memory-image has grown by more than 3 mm, apart from any increase in the extent of the territory lying 'below the threshold.'

These results are not so satisfactory as the earlier series (see Table I) for determining the actual law of the threshold, on account of the increased degree of practice as the experiments proceeded. But they bring out clearly this fact of the growth, or *exaggeration*, of the memory-image. The following explanation, based on direct deductions from Weber's

Law, is suggested to account for this exaggeration. As given here, it assumes Weber's Law to hold rigidly; but even if we accept the latter only in the modified form that *the increments of sensation grow less rapidly than the increments of stimulus*, it will be seen to apply as a constant tendency, and will produce the result indicated, if the supposition on which it is based be admitted.



Let AC be the normal stimulus, and AB, AE the stimuli just perceptibly different from it. Then $\frac{AC}{AB} = \frac{AE}{AC} = r$, a constant for the entire series, according to Weber's Law; and $CE > BC$. Now the central stimulation of the memory-image may assume any magnitude $>AB$ and $<AE$, and any image within these bounds may be identified with the memory-image in respect to size. As there is no objective regulation of the stimulus, the actual representations will tend to distribute themselves, according to the theory of chances, evenly between B and E; but the images around B and E, and decreasingly towards C, will tend to be rejected as too small and too large, respectively. As the memory-image becomes indistinct, however, the imagination-images nearer C are less frequently rejected, and at length no images will be rejected between two given points, M and N. Now since the actual reproductions are distributed evenly between M and N, and none are rejected, the *average* of these will tend to assume the function of memory-image; that is, the point D, midway between M and N, will tend to become the basis of judgment, since AD is the average of the unchallenged images. But, since CN is always greater than MC, the point D will always lie beyond C; *i. e.*, $AD > AC$. Thus there will always be a tendency on the part of the memory-image to grow larger, as soon as there is any tendency on its part to become vague or indistinct; and the continuation of this process will be limited only by the limits of the vagueness of the memory-image, or by its relations to other objects or

memory-images which afford a means of comparison and regulation. In the instance at hand there are no such means of regulation within very wide limits, and the exaggerating process goes on without hindrance, so that in the end the point B is transferred to a position beyond C,—a result which, while unexpected and remarkable, is fully accounted for by the above theory.

The close of the college year prevented an extension of these experiments to intervals of 40 minutes with the same set of men.

A word or two may be in place here regarding the relation between single experiments on a number of subjects and a series of experiments on a single individual. In any experiments where a number of results are combined and their averages taken, what is sought is a *representative* value. By multiplying the trials, accidental influences are eliminated and we obtain a value representative of the given individual under the given conditions. If the individual represents some peculiar type, we should further compare his results with those obtained from individuals of other types. If, however, what we desire is the observation of an *average individual*, we must make sure that our subject is such, by comparing him with others. Rather than repeat the entire series on several individuals, we may save time and labor by performing a single experiment on a number together. There are then a number of precautions to be taken. (1) Each subject must understand perfectly the nature of the judgment to be made. (2) The judgments must be entirely independent. (3) The subjects must be representative—not drawn from some one peculiar class; and they must be governed by sensibly the same conditions. (4) Finally, care must be taken with the *objective* conditions of the experiment, so that no vitiating circumstances shall creep in.—In the present instance, every precaution was taken to fulfil the first two and the last of these requirements, and, a number of doubtful results having been rejected, the remainder fulfilled the conditions exactly, as far as a most careful scrutiny and attention on the part of the two observers could determine. Further, the subjects were acted upon by sen-

sibly the same conditions during the given interval. There is, of course, room for variety of opinion as to how far representative a college class is to be considered, and what allowances, if any, should be made for differences in previous occupation and differences in location with reference to the platform where the squares were shown. The writers are inclined to minimize these differences, and as to the former question, it is urged that a body of men like those under consideration are perfectly representative of the average educated male of about 21. We believe the results to be far more satisfactory than a quantity of experiments on merely one or two individuals, and think that this *cumulative* method, under which alone are possible certain experiments involving a great amount of time, may safely be used in connection with the more usual procedure.

III. THE EFFECT OF SIZE-CONTRAST UPON JUDGMENTS OF POSITION IN THE RETINAL FIELD.

BY J. MARK BALDWIN.

I. *Problem, Apparatus, and Methods.*—The indication given in a preceding Study (II) that the arrangement of squares of various sizes in the visual field has an influence upon the identification of one of them as of a certain remembered size, suggested a farther research. It occurred to the writer that any influence of contiguous squares upon each other would be accurately measured by their joint influence upon the subject's estimate of some other distance on the retina. And such a distance as that lying between the squares lends itself directly to this purpose.

An arrangement was readily effected, whereby the ratio of the sides of two squares to each other was varied in a series of values, while the distance between the squares was kept constant. Any regular variations then in the judgment of this latter distance, such as that of its mid-point,—*i. e.*, the bisection of the distance between the squares,—would be due to the variations in the ratio of the square-sizes. Such a problem shows practical bearings also in all matters which

require estimates of balance, division, proportion in right lines between masses, objects, etc., in the field of vision: such matters as the hanging of pictures, all designing of cuts, vignettes, architectural plans, etc., which involve line values. Of course all variations from the correct location of a mid-point, or other critical point, lying between two masses of material, color, etc., should be allowed for in applying the formulæ of æsthetic effect.

A further complication also arises when movement enters into the case: the movement of the contrasted masses toward or from each other, of the eye from one to the other along the line of connection, or of the element of this line whose evolution describes the line.

Experimental Arrangements.—The following description (with Fig. 1) of my device for investigating the problem is given in some detail, since it meets the essential requirements of such experimentation and is so simple in principle that it may be adopted by others who desire to carry this kind of experimentation further.

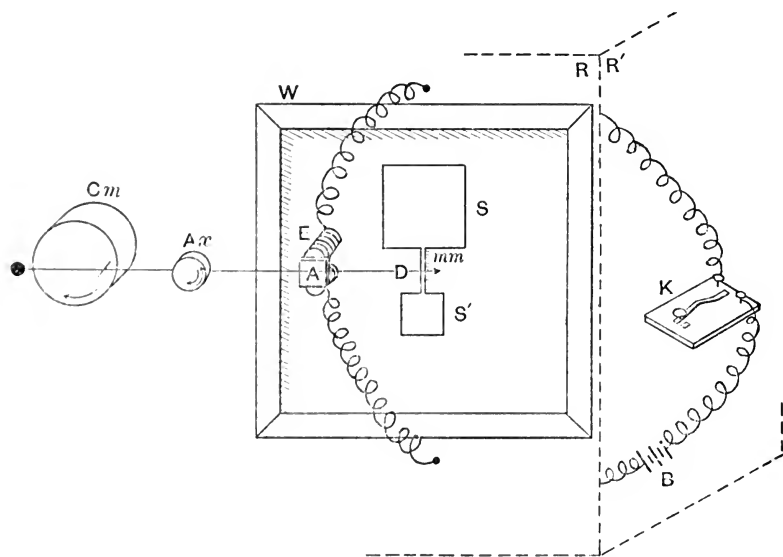


FIG. 1.

The dark room (R) communicates with room I (R') by a single window (W) which is completely filled with white

cardboard. In this cardboard two square holes are cut (S and S¹) whose sides are of determined ratio to each other, and whose distance from each other is measured by a slit (D) bearing a known ratio in length to the side of the larger square. On the wall beside the window (at Ax) is fixed the axis of movement of a long needle which is moved upon this axis by a pin carried round the face of the clock motor (Cm) of a Rothe polygraph. The movement of one end of the needle upward by the pin and downward by its own weight, is reversed by the other end of the needle, which so carries an arrow-head or pointed marker up and down the mm scale marked upon the slit D. The needle bears at A the armature of an electromagnet. The magnet (E) under the armature is fixed to the cardboard and its connections are carried into room R' and terminate in a punch-key (K) on a table directly in front of the window W. The reagent sits at this key, closes the current when the needle reaches the mid-point of the slit D, the needle is arrested by the attraction of the magnet (E), and the reading is given on the scale mm. The apparatus works automatically, giving a series of experiments, with alternating up and down movement of the needle, until the motor runs down. A gas jet in room R is focussed through a large reading lens upon the scale mm, converting the small point of the needle seen by the reagent from the other room, into a moving bead of light. The back-ground of the squares and of the slit is the black of the dark-room wall, and the whole is seen by him upon the white surface of the cardboard.

For the horizontal arrangement of the squares, the whole apparatus is simply shifted 90°, bringing the axis of movement of the needle below the window.

With the arrangements thus described experiments were carried out on two persons; Sh., (W. J. Shaw) and T. (G. A. Tawney), with results as given in this report.¹ Both were practiced in psychological experimentation, but Sh. more than T.

¹ My thanks are due to these gentlemen, as also to two others who gave me some test series. For special reasons the conditions of reaction of the latter were not typical, and so they were not continued.

In the case of each, the series of values of the ratio $\frac{S^1}{S}$ was $\frac{1}{2}, \frac{1}{4}, \frac{1}{8}, \frac{1}{16}$, which gives, when S has the constant value of 20 cm, the following series of values for S^1 , *i. e.*, 10, 5, 2.50, 1.25 cm. A constant value for the distance between the squares was selected which seemed about as likely to occur in ordinary arrangements and experiments as any other, *i. e.*, $\frac{1}{2} S = 10$ cm.

The experiments were performed in series of 20 to 25, called each a 'lot,' only one lot being taken at a sitting to avoid fatigue of the eyes. The time of day was kept constant, the subject was kept in entire ignorance of the object of the research and of the results he gave, and was asked after each series to give any impressions he might have of the accuracy of his results, and of the variations which he made, if any, in his method of identifying the mid-point. Careful record was kept of all these impressions, and they turned out to be valuable.

Methods of Identifying the Mid-point.—The two reagents began at the very beginning of the experiments to describe their procedure differently—a difference which was persisted in and became in the sequel a matter of fundamental importance. Sh. tended to fix his gaze upon the moving bead of light; followed it in its course, and stopped it when it reached the mid-point. This, it is evident, involves an element of eye-movement through a series of positions corresponding in extent directly to half the time D. This I shall call the 'approach method,' since the mid-point is selected only as it is approached by the light-bead.

T., on the other hand, tended to select the mid-point first; and endeavored to hold it fixed until the light-bead reached it, then fixing the bead by his reaction. This evidently gives a result largely independent of eye-movements on the line D, and this may accordingly be called the 'fixation' method. It will be seen below that very important consequences follow from this difference of method.

I. Approach Method. Vertical Arrangement. Results of Sh.—The results of 770 experiments with the vertical arrangement upon Sh., who used the 'approach' method, divided

into 5 series of 6 lots each, are shown in Table I. In the 'vertical arrangement' the larger square was above the smaller in all cases. The variable error is not given in any of the tables, since it fell below the limit of accuracy of the apparatus, *i. e.*, the diameter of the light-bead. The uniformity in direction of the constant error is shown in the small number of exceptions or minus judgments given in the column Excepts. in the table. The words 'down,' 'up,' 'both,' signify the direction of movement of the needle.

TABLE I.—Sh. App. Method. Ver. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excepts.
		Both Directions.	Down.	Up.	
155	20:10	2.35	2.7	2.	6
150	20:5	3.6	3.95	3.2	2
150	20:2.50	3.89	4.27	3.52	0
150	20:1.25	4.4	5.	3.8	0
165	20:0	2.8	2.93	2.66	1

The consideration of the figures given in this table enables us to formulate the following statements for the case in which the eye follows the stimulating bead to its point of arrest, up and down a vertical line:

1. There is a tendency to fix the mid-point too far away from the larger square (positive values of mean var.)
2. The direction of the tendency to error has practically no exceptions.
3. This tendency varies in some direct ratio with the ratio of the sides of the two squares to each other; *i. e.*, from .01215 of the side of the larger square when its ratio to the side of the smaller is 2:1, to .02 of the side of the larger when its ratio to the smaller is 16:1.

4. At the limiting value (0) of the side of the smaller square, the tendency to locate the mid-point too far away from the larger square is about the same as when the sides of the two squares are in the ratio 2:1.

5. The tendency to error is from 16 to 25 per cent. stronger when the stimulating object whose location is fixated is in movement in the same direction as the tendency of error (down), than when it is in movement in the opposite direction (up).

II. Results of Sh. Horizontal Arrangement.—Passing now to the horizontal arrangement, in which the details of apparatus remained the same as for the vertical, I may report as before for the two methods. The larger square was placed to the left, the smaller to the right, and the bead of light moved right and left over the slit between. The variations in the side of the smaller square gave as before the series of ratios to the side of the larger, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$. The following table shows the results:

TABLE II.—Sh. App. Method. Hor. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Expts.
		Both Directions.	Right.	Left.	
100	20:5	.9	1.95	.33	20
50	20:2.5	1.67	2.5	.7	4
50	20:1.25	2.73	3.	2.46	2
50	20:0	2.1	2.	2.2	3

From the examination of Table II we gather the following results:

1. There is a practically uniform tendency of error away from the larger square.

2. This tendency varies in some direct ratio with the ratio of the sides of the two squares to each other.

3. The magnitude of the error is from .9 to 2.2 mm, *i. e.*, .005 to .01 of the side of the larger square.

4. At the limiting value (0) of the side of the small square the tendency is slightly less than when the ratio of the two sides is 16 : 1.

5. This tendency is about $\frac{1}{2}$ greater when the movement of the stimulus fixated is in the direction of the error itself (right) than when it is in the opposite direction (left).

III. Fixation Method. Vertical Arrangement. Results of T.—The results of 683 experiments with the vertical arrangement upon T., who used the fixation method, divided into five series of six lots each, are as follows. See Table III.

TABLE III.—T. Fix. Method. Ver. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excp ^{ts} .
		Both Directions.	Down.	Up.	
84	20:10.	2.96	1.99	3.85	4
150	20:5.	2.86	3.11	2.64	1
150	20:2.5	3.31	2.21	3.83	0
150	20:1.25	2.83	2.35	3.3	1
149	20:0	1.05	.8	1.35	21

Examination of this table enables us to make again the following statements for this subject with the method and arrangement described:

1. There is a tendency to error in the direction away from the larger square.

2. This tendency has so few exceptions that they are due probably to accidental causes.

3. The amount of this tendency is given in a number which fluctuates slightly about a value equal to .015 of the side of the larger square.

4. At the limiting value (o) of the side of the smaller square there is the same tendency to error, but it is less than $\frac{1}{2}$ the error when the ratio is 1:2.

5. The tendency to error is about 50 per cent. greater when the stimulus for fixation is moving in the direction contrary to that of the variation itself than when it is moving in the same direction.

IV. Results of T. Horizontal Arrangement.—The experiments on T. with the horizontal arrangement, his method remaining as before that which I have called the 'fixation method,' gave the results shown in Table IV.

TABLE IV.—T. Fix. Method. Hor. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Expts.
		Both Directions.	Right.	Left.	
100	20:5	1.64	1.91	1.38	4
50	20:2.5	2.7	3.	2.5	0
50	20:1.25	3.25	3.65	2.9	0
25	20:0	2.6	1.53	3.9	0

From the examination of this table we may make the following statement of results for T.:

1. There is a uniform tendency to error in the direction away from the larger square.

2. This tendency is from 1.64 to 3.25 mm, *i. e.*, in this case .008 to .016 the side of the larger square.

3. This tendency varies in some direct ratio with the ratio of the sides of the two squares to each other.

4. At the limiting value (o) of the side of the smaller square the tendency to error is the same as when the ratio between the sides of the two squares is $\frac{1}{2}$.

5. The tendency is about $\frac{1}{2}$ greater when the stimulus fixated is moving in the direction of the tendency to error (right) than when it is moving in the opposite direction (up).

V. Rectification Method.—It is evident that a second series of indications may be obtained from the experiments given above in cases in which the reagent expresses his sense of the correctness or incorrectness of his result in each experiment. Both Sh. and T. were instructed to indicate after each experiment whether or not the bead gave a satisfactory result when stopped, and also in which direction the result should be rectified to give satisfaction. Records were kept of all such indications. Since it involved a secondary fixing of the mid-point, it approaches the 'fixation' method; but since it followed upon the earlier determination made when the needle was in motion, it involves influences akin to those of the 'approach' method; so it may be considered a combination of the earlier methods and a refinement upon both of them, for it requires a second act of judgment or criticism of the result already rendered in each trial. So let us call it the 'rectification' method.

It is further apparent that this rectification of the result of any given experiment may take one of four phases. It may be a judgment that the needle has gone too far, this we may call rectification by 'reversal;' or that it has not gone far enough, rectification by 'supplementing.' And each of these kinds of rectification will include again two instances. There will be reversals when the movement is in the direction of the prevailing error (*i. e.*, away from the larger square), and when the movement is contrary to the direction of the prevailing error (*i. e.*, toward the greater square.) And the same two cases occur for the 'supplemental' rectifications.

The cases of rectification in the experiments on Sh. and T., both of whom were instructed to use the method, may be thrown into the following tables, in which the four kinds of rectification are distinguished.

Results for Sh. Rectification of Results Secured by Approach Method. Vertical Arrangement.—Giving the figures for Sh. in the vertical arrangement we have Table V.

TABLE V.—Sh. App. Method. Ver. Arrgt.

	No. Rects.	Ratio of Sides in cm.	Reversals.			Supplementals.		
			Movt. from S.	Movt. to S.	Total.	Movt. from S.	Movt. to S.	Total.
	15	20:10	1	9	10	3	2	5
	14	20:5	1	7	8	5	1	6
	17	20:2.5	1	9	10	6	1	7
	33	$\left\{ \begin{array}{l} 20:1.25 \\ 20:0 \end{array} \right\}$	5	18	23	6	4	10
Totals . .	79		8	43	51	20	8	28

From this table we may conclude as follows:

1. Of the rectification of results secured by the approach method, the 'reversals' are nearly twice as frequent as the 'supplementals.'

2. The 'reversals' are 5 times as frequent when the bead moves against the tendency to error as when it moves in the same direction.

3. The 'supplementals' are $2\frac{1}{2}$ times as frequent when the bead moves in the direction of the error as when it moves in the contrary direction.

4. Rectifications take place in $\frac{1}{10}$ the entire number of experiments.

Horizontal Arrangement.—The rectifications of Sh. for the horizontal arrangement are shown in Table VI (first line).

TABLE VI.—Hor. Arrgt.

Subject.	No. Rects.	Method.	Ratio of Sides in cm.	Reversals.			Supplementals.		
				Movt. from S.	Movt. to S.	Total.	Movt. from S.	Movt. to S.	Total.
Sh.	38	App.	Whole series lumped.	8	24	32	3	3	6
T.	25	Fix.	"	12	7	19	1	5	6

It results from this table:

1. The 'reversals' number 5 times the 'supplementals' among the rectifications of data derived by the approach method.

2. The 'reversals' are 3 times as many when the bead moves in the direction contrary to the prevailing error (*i. e.*, toward the larger square), as when it moves in the opposite direction.

3. The supplementals are equally divided between the two cases of opposite movement of the bead.

4. The number of rectifications is about $\frac{1}{6}$ of the number of experiments.

Results for T. Rectifications of Results Secured by the Fixation Method. Vertical Arrangement.—The results of T. with the vertical arrangements appear in Table VII.

TABLE VII.—T. Fix. Method. Ver. Arrgt.

	No. Rects.	Ratio of Sides in cm.	Reversals.			Supplementals.		
			Movt. from S.	Movt. to S.	Total.	Movt. from S.	Movt. to S.	Total.
	10	20:10			0	7	3	10
	19	20:5	12	5	17	1	1	2
	19	20:2.5	3	1	4	6	9	15
	31	$\left\{ \begin{array}{l} 20:1.25 \\ 20:0 \end{array} \right\}$	7	9	16	4	11	15
Total . . .	79		22	15	37	18	24	42

Table VII shows the following:

1. Rectifications by 'supplementing' are $\frac{1}{4}$ more frequent than those by 'reversal' when the results are secured by the fixation method.

2. The 'reversals' are $\frac{1}{2}$ more frequent when the bead moves in the direction of error than when it moves in the contrary direction.

3. The 'supplementals' are $\frac{1}{3}$ more frequent when the bead moves in the direction contrary to that of the prevailing error than when it moves in the same direction as the error.

4. The entire number of rectifications is $\frac{1}{9}$ of the entire number of experiments.

Horizontal Arrangement.—The rectifications of T. for the horizontal arrangement are given in Table VII (second line).

1. *Results.*—The 'reversals' are three times the 'supplementals' in the fixation method, horizontal arrangement.

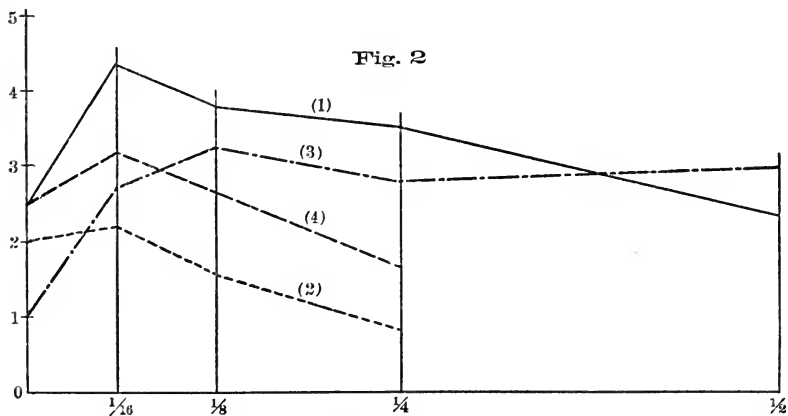
2. The reversals are $\frac{1}{2}$ more when the bead moves in the direction of error than when it moves in the opposite direction.

3. The 'supplementals' are five times more when the bead moves contrary to the direction of error than when it moves in the same direction. This result, however, is based on too small a number of cases to be taken as a numerical ratio.

4. The number of rectifications is $\frac{1}{9}$ of the whole number of experiments.

VI. *General Interpretation of Results.*—We are now able to gather up the results shown in the earlier tables in some more comprehensive statements, based upon the whole number of experiments taken together.

I. Considering the results for the direction and amount



of error without regarding the influence of the direction of movement of the light-bead, we may plot curves showing

the tendency and amount of error for each of the two arrangements by each of the two methods. In Fig. 2 the horizontal ordinate represents the constant series of ratios of the square-sides to each other; the vertical ordinate, the size of the error and its duration (above the abscissa denoting error away from the larger square). Curves (1) and (2) give the results by the approach method, vertical and horizontal arrangements respectively; curves (3) and (4) the results by the fixation method, vertical and horizontal respectively. The location of the various points of the curves is determined in each instance by the figures given in the appropriate table above. The curves are numbered to correspond with the respective tables.

Inspection of the four curves gives certain general results which unite and summarize the results already shown from the separate tables above.

1. The four curves (representing 1,928 experiments) agree in establishing a tendency to error away from the larger square of from 1 to 4.5 mm when the side of the larger square is 20 cm.

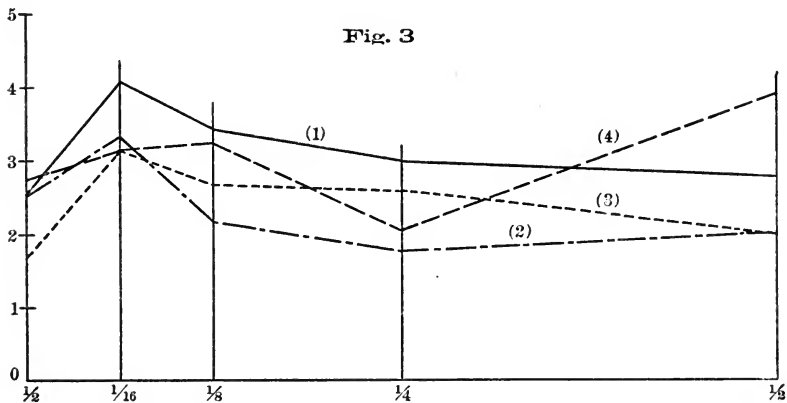
2. The close parallelism of three of the curves in their common direction, and the general parallelism of all the four, establishes the fact that the tendency to error increases with the relative increase of the side of the larger square.

3. The position of curves (1) and (3), considered in relation to the position of curves (2) and (4), shows that the tendency to error, when the squares are arranged vertically, is about twice as great as when they are arranged horizontally.

4. Comparison of curves (1) and (2) with curves (3) and (4) shows that the method of fixation gives more uniform results than the method of approach; and also that the difference in result between the vertical and horizontal arrangements is less when the fixation method is used. It follows from this that eye-movements over a line hinder the correct estimate of the parts of that line, and that this influence of eye-movement is greater for vertical than for horizontal directions.

II. Considering the results with regard to the direction of movement of the light-bead by both methods and in both

arrangements, we may plot the curves of Fig. 3, in which the ordinates remain as in Fig. 2, the points on curves (1) and (2) give the amount of error for the several contrast ratios for the case of movements of the bead away from and toward the larger square respectively by the approach method, and the points on curves (3) and (4) give the amount of error for the same two cases respectively, by the fixation method. These amounts are reached by combining the figures for 'down' and 'right' movements in the tables of vertical and horizontal arrangement of the approach method, for each contrast ratio, and combining similarly the 'up' and 'left' results of the corresponding tables of the fixation method.



Inspection of these four curves (again representing the entire 1,928 experiments) leads us to certain conclusions.

1. Comparison of curves (1) and (3) with curves (2) and (4) shows that the error is greater when the bead is moving in the direction of the error.

2. This is especially the case when the approach method is adopted, the error then being twice as great when the movement is in the direction of the normal error as when it is in the contrary direction [comparison of curves (1) and (2)].

3. It follows that the influence already found to be due to eye-movements varies according to the particular direction of the movement along the line explored. If the eye-movement is toward the larger of the areas contrasted, it tends to cor-

rect the normal error of judgment in the estimation of the time which connects the two areas. If the movement is, on the contrary, away from the greater area it exaggerates the normal error of judgment.

III. The details of the instances of 'rectification' given above serve to confirm these general conclusions, both as to the normal error itself and as to the influence of eye-movements upon it. By the approach method the rectifications by reversal are two to five times more frequent than those by supplementing. This shows that the rectifications in this instance are really corrections of the influences now found to be due to eye-movements. Further, reversals are three to four times as frequent when the bead moves against the tendency to error as when it moves in the direction of this tendency. This shows that these corrections are much more likely in direction opposite to that in which we now find the real contrast error to occur. When moving in the direction of the contrast error the eye-movement influence gets support from that error, and so fails of detection, and even secures supplementing in this direction more frequently than the movement in the opposite direction does. This is an indirect determination of the true direction of the contrast error in agreement with the direct experimental result.

The rectifications in the fixation method, on the other hand, are equally divided between the 'reversals' and the 'supplementals,' showing that the influence of eye-movement is largely eliminated by this method. And further, the distribution of both supplementals and reversals between the two cases of movement, in one direction or the other, is now directly reversed, *i. e.*, the reversals are more frequent when the bead moves in the direction of error, and the supplementals when it moves contrary to this direction, a result which seems to show that in this case the tendency to error from contrast is in conflict with the normal influence of eye-movements, and the correction is made to increase the latter in one direction, and to diminish the former (or their sum) in the other direction.

The entire number of rectifications of all kinds (about $\frac{1}{3}$ of the whole number of experiments) may be taken as a sort

of quantitative indication of the function of second-judgment, or deliberation, upon sensory determinations of such a complex character as those involved in these experiments. It is interesting to note that this second judgment, however, does not tend in the general result to correct the error of first judgment; for there are about $\frac{1}{2}$ more cases of rectifications by displacement toward S^1 (the direction of the error) than toward S . The only case in which the correction does work to give greater accuracy to the result is that of the use of the fixation method, where both the original and second judgments are comparatively free from eye-movements and their after effects.

Finally, the great uniformity of the error of judgment is seen in the small number of cases (69 in the entire series of 1,928 experiments) in which the mid-point was located in the direction opposite to the prevailing error (that is, located too far toward the large square). And even this number represents too high a figure, since the sum of the variations of this kind in all but two series gave only 28 cases (*i. e.*, in 1,679 experiments); the two giving the very abnormally large figures 20 in 100 experiments (app. method, horiz. arrangement) and 21 in 149 experiments (fix. method, vert. arrangement) being evidently affected by some temporary influences.

A series of experiments has already been begun with a stationary stimulus (thus ruling out the influence of eye-movements); and I hope also to complicate the case with variations planned to introduce æsthetic elements into the problem.

IV. TYPES OF REACTION.

BY J. MARK BALDWIN (with the assistance of W. J. Shaw.)

The experiments reported in this article were carried out in the Toronto Laboratory in 1892-93. Three questions were set for research, all of them bearing on the question of the degree of relativity of reaction times: either the difference of a single individual's times, according as there

were subjective (attention) or objective (qualitative stimulus) changes in the conditions of his reaction; or the differences of reaction times for different individuals under identical conditions. To secure results comparable in the respects in which comparisons were desired, certain precautions were made, as follows: (1) each reagent reacted at the same hour from day to day, and at the same hour with each other reagent whose reaction was to be compared with his; (2) the order of change in the conditions of reaction (as sensory-motor, light-dark, visual-kinæsthetic, etc.) was kept in the main the same for the different reagents.

The Hipp and D'Arsonval chronoscopes were used, both controlled by the records of a König tuning-fork recording on the drum of the Marey motor. The 'light' reactions were taken in a room of good south morning exposure, and those in the dark, in a dark closet of the same room. The stimulus was in all cases an auditory one—a sharp metallic click—and the reacting movement was a pressure downward of the right forefinger (in the case of the D'Arsonval instrument, a pinch of that finger and the thumb). The reagents were, besides the writers (B. and S.), Mr. Faircloth (F.), a student who had had only the experience gained from the practical work in this subject of the course in Experimental Psychology. His reactions were ready and unconfused, and from all appearances he was a normal and more than usually suitable man for such work. The fourth, Mr. Crawford (C.), is an honor student in this subject in Princeton. His reactions were taken in the course of another investigation, and being so few in number, they are included only because they give a certain case of a capable reagent whose sensory is shorter than his motor reaction. We hope to test him further.

I. *Variations in the Results.*—Table I. shows the relative reliability of the two instruments in these experiments.

TABLE I.—Clock-corrections.

Instrument.	Const. Error.	Var. Error.
D'Ars.,	.19	.06228
Hipp,	.019	.015616

All the results secured by each instrument are corrected, by the constant error of that instrument, before being used either for comparison among themselves or for compounding with the results of the other instrument, in the tables which follow. The smaller variable error of the Hipp chronoscope makes the results of that instrument much more reliable in the matter of absolute time-measurement. But in the conclusions drawn below, only those results are used in which the quantity sought is a relative one, and in which the two clocks confirmed each other in giving ratios of difference of the two quantities compared, both of which are in the same sense, and each of which is larger than the largest possible ratio of difference arising from the variable error of the clock to which it belongs.

The mean variations are not given in the tables which follow, because they are too complex to be of any value. These variations were different for the two clocks, as we would expect from the variable errors of the instruments themselves; they also varied with the disposition of the subject in the various groups of results which are treated together.¹ The different mean variations for the different lots of experiments varied from 10σ to 20σ . For this reason no deductions are attempted except those evident on the surface of the results themselves.

II. *Results: Sensory and Motor Reactions.*—Table II. gives the results of experiments on four persons designed to test the current distinction between 'sensory' and motor ('muscular') reactions.

¹ Finer distinctions were aimed at in some of the series, such as placing the sound stimulus on one side only, or in the median plane below the head, etc., as well as arranging for the difference between light and dark environment.

TABLE II.—Types of Reaction.

Sub- ject.	No. Exps.	Sensory.		Kin. Motor.		Vis. Motor.		Av. Motor.	
		No.	Time in σ .	No.	Time in σ .	No.	Time in σ .	No.	Time.
B.	2490	1043	178	966	149	481	171		160
S.	2572	1017	235	995	184	560	195		187
F.	820	290	164	260	202.3	270	205.5		204
C.	212	84	132					128	157

It follows from Table II.: (1) that the current distinction between sensory and motor reactions does not hold in the sense that the motor reaction is always shorter than the sensory, for in the case of F. the motor reaction is 40 σ longer, *i. e.*, $\frac{1}{3}$ of this subject's average sensory reaction time. (2) As between B. and S., in the case of each of whom the motor-time is shorter, there is a great difference in the relative length of the sensory to the motor. In B. the sensory time is only 18 σ , or about $\frac{1}{3}$ longer than the motor, while in the case of S. the sensory is 48 σ longer, or about $\frac{1}{4}$; and this despite the close agreement of the two subjects in their absolute motor-time. We would seem to have, therefore, in these three observers three cases shown, two giving very pronounced results; one a longer motor time by $\frac{1}{3}$, and the other a longer sensor by $\frac{1}{4}$. The third subject, B., seems to fall between these extremes, giving a difference in favor of the motor reaction, it is true, but a much smaller difference.

The tables also give us reason for accepting the truth of the distinction between two kinds of motor reaction. In both B. and S., whose motor reactions are shorter than the sensory, we find a difference in the length of the motor reaction according as the attention is given to the movement by thought of the hand, the eyes being blindfolded; or as the attention is fixed upon the hand, which is seen. The former I have called the *kinæsthetic motor* reaction, the latter

the *visual motor*. In B. the visual motor is 22σ , or about $\frac{1}{4}$ longer than the 'kinæsthetic'—that is, it is practically equal to this subject's sensory time; while in S. the kinæsthetic motor is 11σ shorter than the 'visual.' With F., on the contrary, there is no distinction between the two kinds, any possible trace of it seeming to be lost in the excessive preponderance, in facility, of the sensory kind of reaction.

The table as a whole, then, supports the views: (1) when the motor reaction is short in relation to the sensory (case of S.), then this motor reaction is purest, freest from sensory influences, such as sight, etc.; (2) when the motor reaction is not pure, then it is retarded by such influences as sight (case of B.); (3) where the motor reaction is relatively difficult and delayed, as compared with the sensory (case of F.), there this prime difference renders all kinds of motor reactions equally lengthy and hesitating. B. seems to stand midway between the two others in this respect.

As I said some time ago, in making a first report upon the outcome of some of these experiments:¹ "In subjects of the motor type the 'kinæsthetic motor' is shorter, the 'visual motor' time approximating the sensory reaction time."

III. *Light and Dark Reactions to Sound*.—The foregoing deductions concerning the difference between B. and S., as respects motor and sensory reactions, and also as respects the distinction between visual and kinæsthetic motor reactions, are confirmed by results of a research on the same two subjects, in which the attempt was made to investigate the influence of vision. Each reagent gave a series of reactions in the light of an ordinary laboratory room, and then repeated the series under the same general conditions, but in a dark chamber. In this case, in order to make the results of the two series comparable, the kinæsthetic form of motor reaction was necessary in the series taken in the light, since only that kind of motor reaction was possible in the dark. The results are given in Table III.

¹ New York *Medical Record*, April 15, 1893, p. 455 f.

TABLE III.—Reactions in Light and Dark.

SUBJECT.	LIGHT.				DARK.			
	Sensory.		Motor.		Sensory.		Motor.	
	No.	Av. in σ .	No.	Av. in σ .	No.	Av. in σ .	No.	Av. in σ .
B.	541	176	979	164	502	184	468	138
S.	537	237	1190	158	480	219	469	179

On examination the data of this table, compared with those of the preceding table, may be stated as follows: We find for B. that the sensory reaction is practically the same, whether he react in the dark or in the light (the latter is less by 8σ , which is insignificant in view of the variable error). This shows this subject's independence of vision in the sensory reaction to auditory stimulations, and is in agreement with the results of Table II. (in which there is a similar difference between the sensory and visual motor, the former being longer by 7σ). S., on the other hand, shows a shortening of the sensory reaction when in the dark by 18σ , but a lengthening of the motor reaction when in the dark by 21σ , or about $\frac{1}{4}$. The latter result shows this subject's dependence upon vision only in the motor kind of reaction.¹

IV. *Interpretation.*—Admitting that these results indicate clearly the existence of persons whose sensory reactions to sound are shorter than their motor reactions, and that there is in some individuals a difference between the length of the motor reaction, according as it is made in the light or in the dark, we may make some general remarks on the theory of these differences. These results should be compared with earlier ones, a matter which is made easier by reference to the concise summing up of the literature of the subject by

¹ The 'dark-reaction' was not secured from F., the 'sensory' subject; but we hope to report further results obtained from C., the similar case now found in Princeton.

Titchener in *Mind*.¹ We find cases of relatively shorter sensory times similar to mine reported (for electrical stimulus) by Cattell,² and (for sound stimulus) by Flournoy³. We may accordingly say that such individual differences are clearly established, and must hereafter be acknowledged and accounted for in any adequate theory of reaction.

The attempt of Wundt, Külpe and others to rule these results out, on the ground of incompetency in the reagents, is in my opinion a flagrant *argumentum in circulo*. Their contention is that a certain mental *Anlage* or aptitude is necessary in order to experimentation on reaction-times. And when we ask what the *Anlage* is, we are told that the only indication of it is the ability of the reagent to turn out reactions which give the distinction between motor and sensory time, which Wundt and his followers consider the proper one. In other words, only certain cases prove their result, and these cases are selected because they prove that result. It is easy to see that this manner of procedure is subversive both of scientific method and of safely-acquired results in individual psychology. For the question comes: what of these very differences of individual *Anlage*? How did they arise; what do they mean; why do they give different reaction-time results? To neglect these questions, and rule out all *Anlagen* but one, is to get the psychology of some individuals and force it upon others, and thus to make the reaction-method of investigation simply the handmaid to dogma.

The attempts to explain the relative shortness of the 'muscular' reaction, also, by those who hold its shortness to be a universal fact, have been unfortunate. It has been

¹ Jan., 1895, p. 74.

² *Philos. Studien*, VIII., 403.

³ *Arch. des Sci. Phy. et Nat.*, XXVII., p. 575, and XXVIII., p. 319. Titchener, in his summing up, does not cite the cases of Flournoy nor the earlier report of one of my present cases (F.) in the *Medical Record*, Apl. 15, 1893, although they tell directly against his own views. My earliest case was noted by me in the autumn of 1892, and the note in the *Medical Record* was written in December, 1892, before I saw either Cattell's or Flournoy's articles. The sentences quoted from my *Senses and Intellect* by Titchener in *Mind*, *loc. cit.*, were based upon my own reaction-times, taken earlier when I had no reason to doubt the universality of the experience, as claimed by Lange and Wundt. Titchener is accordingly wrong in citing me as favoring that position.

held that the muscular reaction is shorter because it is semi-automatic; the thought of a movement, *i. e.*, attention to it, being already the beginning of the innervations necessary to its production. This is very true as a principle, I think; but it is just the application of this principle which makes it necessary on the part of some to restrict reaction work to people of a special aptitude. For in all those cases, either of particular reactions in one individual or of all reactions in other individuals, in which the movement is not so clearly picturable as to be firmly and steadily held in the attention, to these cases the principle does not apply. On the contrary, to all cases where it is difficult to get the attention fixed upon a *motor* representative of the movement, a very different principle applies, as Prof. Cattell has said. The very attempt to picture a movement *as a movement*—by putting the attention on its *motor* aspect in consciousness—embarrasses, confuses and delays the execution of that movement in these cases. If a marksman attend to his finger on the trigger he misses the target; if a ball-player attend to his hands he ‘muffs’ the ball; if a musician think of each finger-movement he breaks down. The musician in the laboratory is usually, indeed, a glaring instance of unsuitable *Anlage*!

So it is evident that these two principles need reconciling in their application to reaction-times, the principles, *i. e.*, (1), that *the thought of a movement already begins it, facilitates it, quickens it*; and (2) *that attention to a practised movement, in many instances, embarrasses it, hinders it, lengthens it*.

Now the practical reconciliation of just these two principles has been made in another great department of fact, and the actual plotting done of the cerebral arrangements which underlie them—a solution which has such evident application here that I wonder at its tardy appreciation. I refer to the work in the pathology of aphasia, and the general theory of mental ‘types’ which now goes for a safe discovery in the discussions of ‘internal speech,’ ‘sensory *vs.* motor defects’ of speech, etc. I published early in 1893¹ an hy-

¹ See the *Medical Record* (N. Y.), *loc. cit.*

pothesis to account for the variations in this matter of reaction-time differences, in these words:

"I have endeavored incidentally, in an article now in print for the July issue of the *Philosophical Review*,¹ to account for the conflicting results of experiment in this field, by borrowing from the medical psychologists the results of their brilliant analysis of the speech function, on the basis of its pathology. The recognition of the great forms of aphasia—*i. e.*, sensory and motor—and the corresponding recognition of the existence of visual, auditory, and motor speech types, gives a strong presumption that the distinction between sensory and motor in the voluntary movements of speech and writing applies as well to voluntary movements of all kinds; that is, to all movements which have been learned by attention and effort. This means that a man is an 'auditive,' or a 'visual,' or a 'motor' in his voluntary movements generally. His attention is trained by habit, education, etc., more upon one class of images than upon others, his mind fills up more easily with images of this class, and his mental processes and voluntary reactions proceed by preference along these channels of easiest function.

"If this be true, it is evident that a man's reaction-time will show the influence of his memory type. The motor-reaction we should expect to be most abbreviated in the man of the motor type; and less abbreviated, or not so at all, in the 'visual' or 'auditory' man. And experimental results must perforce show extraordinary variations as long as these typical varieties are not taken account of. We are accordingly, I think, a long way off from any such exact statement of absolute difference between sensory and motor reaction-time as Wundt makes in his last edition."²

It was a sense of the great naturalness and probability of this hypothesis that led me early in the fall of 1892 to institute the experiments on 'visual' and 'kinæsthetic' motor reactions whose results are given above in this paper.³

The secure establishing of cases which show sensory reactions shorter than motor (*i. e.*, the cases now reported by Cattell, Flournoy and myself), together with the probable

¹ Article entitled "Internal Speech and Song," *Phil. Rev.*, July, 1893.

² *Physiologische Psychologie*, 3d ed., II., p. 261 ff.

³ At the Philadelphia meeting of the American Psychological Association, on Dec. 28, 1892, I proposed the hypothesis informally to several American psychologists. Dr. Lightner Witmer will remember a conversation in which the point was remarked upon. I venture to make these personal explanations since a somewhat similar explanation of his cases was advanced by Prof. Flournoy, of Geneva, in the articles cited above. I was not acquainted with Prof. Flournoy's views until, a year later at the New York meeting of the Association, Prof. Cattell brought them to my attention, as given in abstracts in the *Revue Philosophique* and the *Zeit. für Psych.* I return to Flournoy's position later on in this paper.

distinction between the 'visual' and 'kinæsthetic' forms of motor time, make it advisable that this hypothesis should be put in clearer evidence. I shall therefore proceed to state the case for it briefly on the basis of the facts as they are now known.

The doctrine of 'types' rests upon certain facts which may be briefly summed up. A voluntary motor performance—say speech—depends in each particular exercise of it, upon the possibility of getting clearly in mind (*intérieur, innerlich*) some mental picture, image, presentation, which has come to stand for or represent the particular movements involved. This mental 'cue' or representative may belong to either of two great classes: it may be a 'sensory' cue or a 'motor' cue. People are of the sensory type or of the motor type for speech according as their cue in speech is sensory or motor; that is, according as in speaking they think of the sounds of the words as heard, the look of the words as written, etc.—the cues furnished by the special senses associated habitually with speech—this on the one hand; or according as, on the other hand, they think of or have in mind the movements of the vocal organs, lips, tongue, etc., involved in speech. In the 'motor' people there are incipient movements in mind; in the 'sensory' people there are special sense images in mind. All this is now so clear from the pathological cases examined that the theory of localization of brain areas and their connections is applied to the successful exploration of damages of the brain when aphasic symptoms furnish the main diagnostic resource.

Now, let us see how in these cases of aphasia the two principles spoken of are applied. Suppose we agree with the neurologists in saying that the function of the 'cue'—the mental image, be it either motor or sensory, which when thought of enables a man to speak—is to release energy from its own brain-seat, along association fibres or pathways, to the motor-seat which sends its discharges out to bring about the movement. Then the difference between sensory and motor people is simply that different centres—different 'cue'-seats—have these connections with the motor speech centres best or better developed. A man who speaks best

when he thinks of the sounds of the words has his best 'cue'-seat in the auditory centre; and his best pathway to the speech motor-centre goes out from this 'cue'-seat. For the man who speaks best when he thinks of the utterance of words, the same may be said of the muscle-sense seat.

So it is evident—apart quite from the question as to how one or other state of things comes to be as it is in any one case—that with one man attention is directed to the *movement* for the best results, with another to the sensation or special memory image in best association with the movement. With the former *the thought of the movement begins the movement*. But with the other, if the best doing of the movement comes from thinking of a sensation or special image, *then the movement will be relatively deranged, embarrassed, when the attention is drawn from this sensation and forced to fix itself upon the movement itself*. These, then, are the two principles we desiderate, and they are both natural parts of the 'type' theory.

So why not generalize this? Speech cannot be considered an exceptional function in its rise and mechanism. Other complex motor functions show the same kinds or types of execution: handwriting, music performing, etc.¹ The hand has, next to the tongue possibly, the most delicate, varied and differentiated functions to perform; and the laws of association by which sensory cues, checks, controls, are affixed to hand actions and combinations, must be the same as those involved in speech. So in simple hand movements people must show the sensory and motor types. This is my hypothesis.

The man, therefore, who gives relatively shorter motor reactions is a 'motor' in his type; with him the thought of movement is the most facile beginning of the movement, just because it is *really the movement*, and nothing else, that he thinks of. That is his *Anlage*. But the man who gives relatively shorter sensory (auditory, visual) reactions, is a 'sensory' in his type: with him the attempt to think of the

¹ See my *Mental Development: Methods and Processes*, pp. 91 ff., and 438 ff. In Chap. XIV. of that work, on 'The Mechanism of Revival,' I have endeavored to put in evidence the general principles which underlie the type theory.

movement *as a movement* interferes with the prompt and exact execution of it, just because he is not accustomed to execute his movements in that way. That is his *Anlage*. But, of course, the two sorts of people have equal claim to recognition in science. Suppose a dead aphasic brought for autopsy to a surgeon, who inquires into the life-history of the man, and finding that he was of the sensory type, then declares that the body is not fit for a scientific autopsy, because the man did not have the proper type of aphasia! As a matter of fact, so near are the disciples of Wundt to the explanation by types that it is only necessary to translate their word *Anlage* by 'type,' and then apply the connotations of that term in the examination of refractory cases, to bring them into line. I may accordingly sum up in the words of my earlier article (*Philos. Rev.*, II, 395):

"We have in this fact of types the explanation of the contradictory results reached by different investigators in the matter of motor reactions. Some find motor reactions shorter, as I have said above; others do not. The reason is, probably, that in some subjects the 'sensory' type is so pronounced that the attention cannot be held on the muscular reaction without giving confusion and an abortive result. On the other hand, some persons are so clearly 'motor' in ordinary life, that sensory reaction is in like manner artificial, and its time correspondingly long. And yet again others may be neutral as regards sensor or motor preferences. If this be true, another element of 'abounding uncertainty' is introduced into all the results of experiments so far performed in this field, as reflection on the matter will show."

One or two further points, however, may be made which give the correct interpretation more importance than the simple facts in themselves really have. In the first place, an additional tendency seems to show itself when movements become very habitual—a tendency recognized in all discussions of the principle of habit. Habitual performances tend to become independent of consciousness, attention, thought, altogether. This tendency should make itself evident in reaction-time work, and reagents of great practise should show, (1) diminishing time in all reactions, and (2) diminishing difference between the two kinds of times, sensory and muscular. Further, the same tendency should show itself in a diminishing difference between individuals

of different types as they both get more practise. All these results are, I think, clearly shown in those of the earlier researches in which the amount of practise is reported.¹

And, again, finer distinctions of type follow from the general theory: such distinctions as those clearly established for speech. The 'visual,' 'auditory,' and possibly (as in the blind) 'touch' types of speech are all included under the head of sensory. As I have said, the speech case *is* a case of finer reaction-time distinctions. And the hand, as used in most reaction experiments, ought to show to a greater or less degree similar distinctions.² The cases so far discovered of relatively shorter sensory reactions seem to be, as far as reported, auditory (musicians) and visual (Flournoy's). To determine between 'visual' and 'auditory' times for any individual, of course the same set of reaction experiments should be made with the two classes of stimulations, each being compared with the muscular reactions to the same stimulus respectively.

The general result follows (if this hypothesis get acceptance) that the reaction-time experiment becomes of use mainly as a *method*. Distinctions supposed to be established once for all by various researches must be considered as largely individual results, inasmuch as the authors have not reported on the type of the reagent. But for that very reason these results may have great value, as themselves indicating in each case this very thing, the type of one single reagent, and in so far some of the general characteristics of that type. What we now desiderate in a great many departments, as, for example, in the treatment of school children, and in the diagnosis of complex mental troubles, is just some method of discovering the type of the individual

¹ Consequently it does not do to say, with Wundt and Külpe, that the 'muscular' reaction is more automatic. Of course it is so in those who give a shorter motor reaction—that is sufficient proof of it. But that the sensory time is shorter in others is sufficient proof, also, that in their case the sensory reaction is more automatic. Külpe's two-arm reaction experiment is subject to this criticism, among others (see Wundt, *loc. cit.*, p. 325; Külpe's *Grundriss*, pp. 422 f.).

² A possible instance of such variation is seen in the case of Donders, which Wundt has difficulty with (*Phys. Psych.*, III^o, II., p. 268). Say the reagent was 'visual' in his type, and we have reason for his shorter reaction to light than to sound, while he still falls under the sensory type in general.

in hand. If reactions vary in certain great ways, according to the types which they illustrate, then in reaction experimentation we have a great objective method of study. But before the method can be called in any way complete, there should be a detailed re-investigation of the whole question, with a view to the great distinctions of mental type already made out by the pathologists.¹

A word should be added concerning the position of Professor Flournoy. The hypothesis which I have advanced has been attributed also to Flournoy, his name being connected with mine as independent advocates of it (by Cattell, Titchener, etc.). I think this is a mistake, at least so far as the publications of Professor Flournoy are taken as evidence. His case, cited as of the 'type visuel,' seems to imply the existence of other types it is true; and at the close of his second article he raises the question, "si la façon de reagir observée chez M. Y., n'est qu'une singularité individuelle, ou si elle est un fait général et constant dans le type visuel d'imagination." But what he means in the context by 'type visuel' is not what is meant by that phrase in the generalized usage of the pathologists. His case is 'visual' in the sense that the man thinks of movements by a visual picture of his arm, rather than by muscle-sense images (just what I have distinguished above as 'visual motor' in distinction from 'kinæsthetic motor;' and the case is a fine confirmation of the conclusions given above under that head). But it does not follow that the man is a 'visual' in the broader sense. He might just as likely be an 'auditive.' The most that can be said of Flournoy's case, on the general doctrine of types—other evidence aside—is that he is 'sensory,' and on my theory his shorter sensory reaction-time proves it. But Flournoy makes no such general application of the theory of types. Indeed, in asking the question which I have quoted from him (*i. e.*, whether all visuals would react as this man did), he shows that he does not mean

¹ I have earlier indicated (*Med. Record, loc. cit.*, and *Philos. Rev., loc. cit.*), the possible use of this method by brain surgeons, an idea which Wallaschek comments on with approval. Certain general indications from reaction-times are already recognized by physicians, especially in investigating various anæsthesias.

to bring reactions generally under the type theory. For the real 'visual' might give a shorter 'visual motor' than 'sensory' time—*i. e.*, when the stimulus reacted to is other than visual (say auditory); since then the *visualizing habit* would throw its influence on the side of the motor reaction.

In the matter of the distinction between 'visual motor' and 'kinæsthetic motor' reactions, however, Flournoy's case clearly anticipated mine in print.¹

V. SENSATIONS OF ROTATION.

BY H. C. WARREN.

The following experiments were undertaken in connection with some other series, with a view to determining the manner in which conflicting data from different senses are harmonized. As they have, in addition to this, a special bearing on the sense of rotation, it seems best to give their results separately. The particular object of this investigation was to determine the relative influence of sight and the internal sense of rotation on the subjective estimate of movement. Messrs. W. J. Shaw and G. A. Tawney kindly acted as subjects during the entire series, which consisted of sittings about 20 minutes long, twice a week, extending over a period of four months in the early part of the year 1894.

The experiments were conducted in a dark room, 8x8 ft. The subject lay at full length on his back on a rotation-board, with the head somewhat raised and eyes so screened as to permit of his seeing only a small area of wall in front. At the foot of the board, and covering entirely his restricted field of vision, was a large screen with an aperture eight inches square, behind which a mirror could be hung. On two opposite walls of the room were hung strips of white paper, an inch wide, reaching nearly from ceiling to floor, and placed about a foot apart, which could be seen plainly in a very faint light, when no other outlines or shadows were

¹Since revising the proofs of this article I have received a note from M. Flournoy in which he says: "Je suis, d'une façon générale, d'accord avec vous sur l'influence du type d'imagination" (making reference to my article in the *Medical Record*).

distinguishable on the screen or wall. When the subject closed his eyes, or the board was turned so that no strips were visible, the usual phenomena of rotary sensation were observed; the least discernible movement was 1° a second; a movement greater than this, if continued, gradually ceased to be noticeable, and any change in the rate was then interpreted as a new movement starting from a state of rest. But when the subject, believing himself to be at rest, was turned so that the strips came into view, a conflict arose between the internal sense and the visual sense, which had to be reconciled by some mode of interpreting the data. The experiments included trials both with and without the mirror, the subject never knowing whether the mirror was in or not.¹

(1) With the minimum light by which the strips were discernible, the sense of sight played no rôle whatever in the judgment, the strips appearing sometimes to flit across the field of vision, and sometimes to move with the subject, according to the data furnished by the internal sense. (2) With an illumination ranging from one to six candles, visual impressions strengthened the internal sense of movement when they agreed, or tended to inhibit it when the conflict was not too great. Thus, with the mirror removed, the sight of the strips made even the slightest movement perceptible, and checked the sensation of reversed movement which occurs after a real movement ceases. With the mirror in, the least perceptible movement was between 1° and 2° per second. Movements greater than this were usually interpreted (when the strips were visible) as lateral movements of *progression in the direction in which the head was actually moving*. A sudden stop converted this into a judgment of *rotation in the opposite direction*. Sometimes, however, especially with very rapid movements, they were

¹ One of the subjects (T.) did not know of the mirror at all until the series was nearly completed. S. generally could not tell whether it was in or not; I questioned him at the end of each sitting, and found that, as a matter of fact, he never considered it in making his judgments, being too busy observing and reporting. T. was more inclined to dizziness than S., and his experiences were consequently less distinct and his answers less uniform. In general the two subjects agreed. I also confirmed a number of the results here given by experiments on myself.

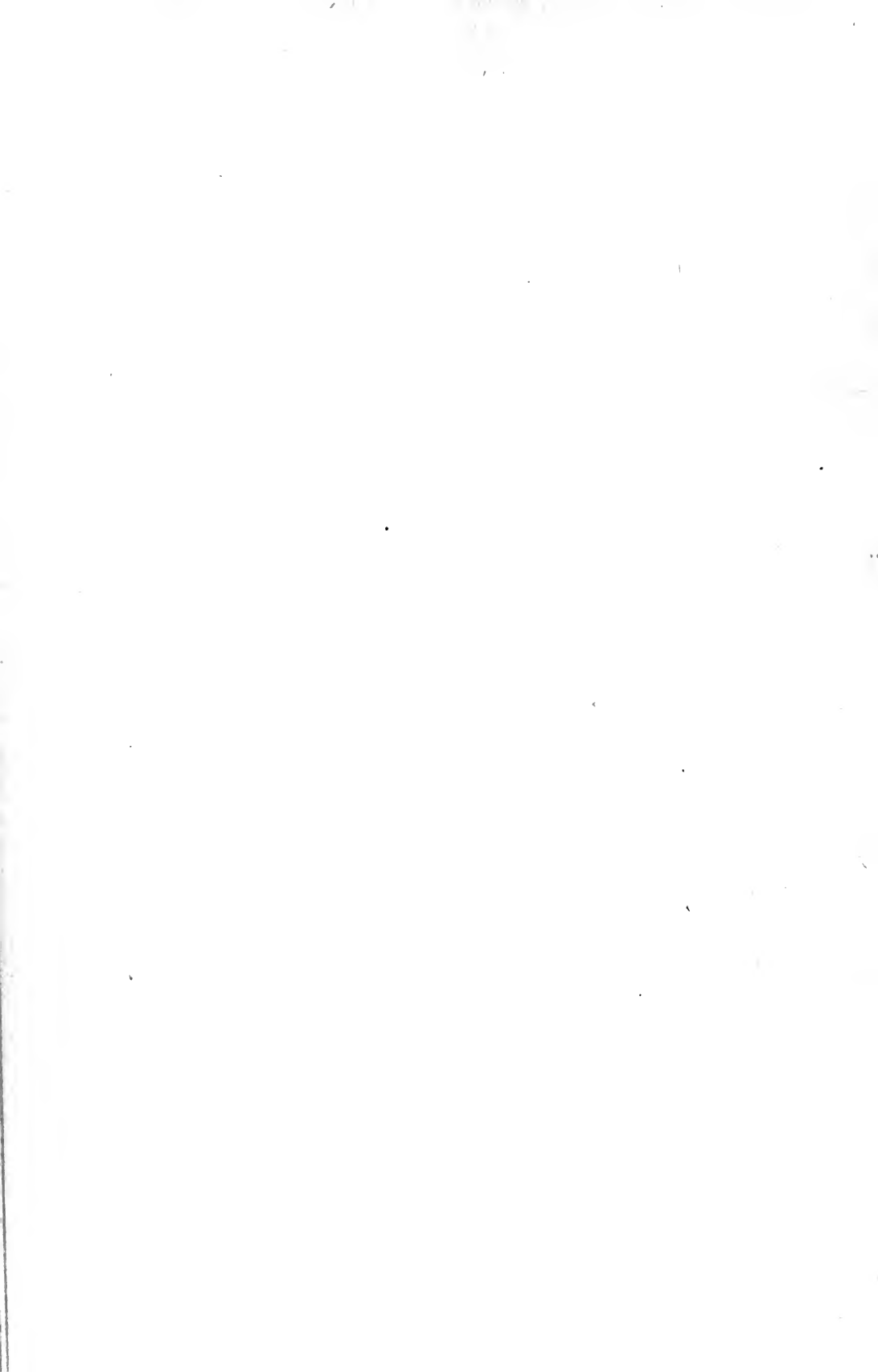
interpreted as rotary, and the strips were declared to be moving also, but faster than the subject. (3) With a bright gas jet burning, the corners of the walls and many other outlines were plainly visible, and gave more general indications of a stable environment than the strips afforded. The judgment of *progressive movement* now occurred uniformly, except when the board was started with a sudden jar; the subject was unable to rid himself of the illusion; he would seem to be moving steadily sideways, even though he knew the impossibility of doing so in a small room. The writer confirmed these results personally. (4) In the final series the subject sat upright at the center of the board, with his head slightly in front of the center of rotation. In this position the interpretation of the movement as progressive is impossible, if the internal sensations in the head are regarded; and as a matter of fact no such judgment was given. Careful and repeated measurement by the metronome gave the following results, with the mirror in and a very strong light (two gas-jets): (a) Movements of less than 2° per second were judged to be objective merely. (b) Movements of 2° – 5° per second were interpreted as subjective, but in the *reverse direction* from that in which they were actually occurring; *i. e.*, they were felt as movements, but their direction was determined not by the internal sense, but by sight, and they were thought to be in the opposite direction on account of the mirror. (c) Movements greater than 5° per second were interpreted correctly, with the remark that the wall was moving also in the same direction as the feet, and faster. The transition from (b) to (c) was very distinct; several times the speed was varied during the trial from greater to less than 5° , or vice versa, and each time there was an immediate change of judgment as to the direction of the movement. A single 'trial' in this case was always limited to 10 sec., in order to avoid the feeling of 'slowing up' which accompanies uniform movement.

Aside from the last-mentioned phenomenon, which belongs more properly to the general subject of conflict among sense impressions, the most noteworthy result was the transformation of rotary into progressive movement, by means of

visual data. Suppose the actual movement to be clock-wise. The head moves to the left, the feet to the right, and the strips and wall reflected beyond the feet are carried much faster to the right. But only the difference between the rate of the feet and the rate of the back-ground is taken into account; the back-ground is considered stationary, and the movement is interpreted as a movement of the subject bodily to the left. We may infer from this that the end-organ of the internal sense of rotation is in the head alone, since movements of the lower extremities are open to such absolute misjudgment. We are also led to the conclusion that the organ for the sense of rotation is the same as that for progressive movement. This is directly contrary to the conclusion reached by Delage, who denies that the sense-organ for progressive movement is in the head, while admitting it for rotary movement.¹ These results, moreover, seem to favor the view that the semi-circular canals constitute that organ, in spite of the objections recently raised by Ayers and others on morphological grounds.² A special sense-organ seems requisite for such a sense, and the above results tend to locate that organ in the head. In our experiments the sense of sight was made to furnish data of movement independent of the internal sense. In the *head* the latter sense was so strong that, within the given limits of visual field and luminosity, a conflict between the two senses invariably resulted in its favor, and any movement observed merely visually was attributed to the objects in the environment. Yet when the *lower extremities* were moving quite rapidly in one direction they were constantly declared to be moving in the other, on the testimony of visual data. The 'internal sense' of movement must therefore be due to something other than the general indication furnished by the vaso-motor system, which would affect all parts of the body alike.

¹ Physiologische Studien über die Orientirung, von Delage; deutsch von H. Aubert; pp. 94-95.

² Journal of Morphol. 1892, VI, pp. 237-256.



SENSORY STIMULATION BY ATTENTION.

BY PROFESSOR JOHN GRIER HIBBEN,

Princeton University.

The important function which is attributed to attention in the processes of sense-perception is very strikingly illustrated in an interesting instance which has recently been brought to my notice, and which throws into sharp relief the phenomena of attention operating in an intensified manner, and consequently modifying sensation to an extreme extent. It very often happens that normal tendencies are most clearly exhibited in unusual or abnormal cases, because there then occurs an inhibition of counter or complimentary tendencies, thus presenting to the observer the unique operation of an undisturbed and unmodified function. Instead of the resultant of many, there is the sole functioning of the one element separated from its usual concomitants. A force thus revealed in isolation is more readily appreciated whenever seen conjoined with accompanying forces in any system however complex.

The instance I wish to present seems to me to be of this nature, a case where the normal functioning of the attention is intensified in a very unusual degree. It is the case of a child about eight years old, a little girl, who as a baby was supposed to be congenitally deaf, as she gave no evidence of hearing any sounds whatsoever. Somewhat later in her development, however, it was noticed that at certain times she seemed to hear, and this always in connection with some objects of special interest, as bright pictures, toys, etc. And this now characterizes her general ability to hear: whenever the subject is one especially interesting to her, she hears without great difficulty; but whenever there is no interest in conversation, it is with greatest difficulty that she can be made to hear at all; and it is impossible to gain her

attention by any sounds, however loud, if she is engrossed in any absorbing task or play. Connected with this naturally there was an extremely tardy beginning of speech and very slow development, though during the last year—her eighth year—there has been a marked acceleration of her progress in this particular. In Preyer's classification of the imperfections and derangements of speech there is no precise place for such a case as this, as it is neither peripheral deafness, nor yet does it seem to be any cerebral derangement.¹ The difficulty seems to be psychical rather than physiological.

The facts as I have given them were narrated to me by a trained nurse, graduate of the New York Hospital Training School, who was in my family for a month or more during this last winter. She had been for several months in the family attending the mother of this child, and had had abundant opportunity for observation and for acquiring accurate information upon the subject. Moreover, she is a woman of very unusual ability, as one of the visiting surgeons of the New York Hospital assured me, and therefore one whose account can be wholly relied upon. In addition to her report of the case, however, I called upon a physician in New York who had known and observed this child for several years, and he fully corroborated the statement as made by the nurse in all particulars. I learned from him also that the child had been examined three different times by an eminent specialist in New York, and no defects either in the outer or middle ear could be detected. The indications all pointed to normal peripheral conditions, and the marked variations in hearing seem due to the central fluctuations of interest and the corresponding concentration or distraction of the attention. This appears only as an exaggerated form of the normal affect of interest upon attention. As Bradley² says: "If an idea engrosses, then any sensation which is connected with that idea may in consequence engross. And attention so far has appeared to consist in interest, either direct or transferred; an account which, we shall find, will

¹ *The Development of the Intellect*, p. 36 ff.

² *Mind*, Vol. XI, p. 310.

hold good everywhere" (*Cf.* Waitz, *Lehrbuch*, 634-7). In the ordinary phenomena of hearing, we recognize two moments, the external stimulus and the inner adaptation of attention. According to Prof. James,¹ "the natural way of conceiving this is under the symbolic form of a brain-cell played upon from two directions. Whilst the object excites it from without, other brain-cells, or perhaps spiritual forces, arouse it from within. The latter influence is the 'adaptation of the attention.' The plenary energy of the brain-cell demands the coöperation of both factors. Not when merely present, but when both present and attended to, is the object fully perceived." Now, in sense-perception the two moments, stimulus without and attention within, are normally so related that the former generally predominates, and is capable of arousing the activity of the latter, which is thus in a sense a function of the former, always reacting when the stimulus is sufficiently intense. In the case which we are considering, however, the attention does not function in an instinctive manner in response to an outer stimulus, and seems independent of its degree of intensity; but is readily aroused by the inner interest, and then alone is the consciousness of the outer stimulus rendered possible. This child, for instance, understands the sign language, and that is resorted to in order to communicate with her until interest has quickened the attention, and that in turn has stimulated the hearing. This is similar, in a much lessened degree, to the ordinary cases of what Herbart styles apperceptive attention—viz., where strained attention brings to consciousness external stimuli otherwise unnoticed. And this is similar also to what Prof. James² refers to as the 'ideational preparation' for sensation, in which, of course, there is increased attention, reinforced by the dominant idea present in the mind. The function of attention in sense-perception is illustrated by Wundt³ with weak auditory stimuli, as the ticking of a watch at some distance from the ear, so that it can be perceived only with some strain of attention.

¹ James, *Psychology*, I, p. 441.

² *Psychology*, Vol. I, pp. 433, 439.

³ Wundt, *Human and Animal Psychology*, pp. 256-7.

but will fall below the limen of consciousness without any relaxation. At intervals of three or four seconds the regularly recurring impressions alternately appear and disappear. In this child's case, however, the attention is not merely a factor necessary to discriminate concerning very weak stimuli, but the very strongest stimuli cannot excite the attention through reaction; it can only be centrally aroused; the hearing, then, being a function of the attention in her case, rather than the two being complementary functions determined by a law of action and reaction.

Moreover, a child's attention is characterized ordinarily by an extreme susceptibility to the sights and sounds of the outer world, and responds almost instinctively to sensorial stimuli. Sustained concentration of attention in childhood is unusual.¹ It is in mature age that attention follows our permanent interests, and only those objects associated with such interests find place in consciousness. Absorption in contemplation occurs only when a large group of associations have for years been forming about the controlling interest. And even then, with interests of greatest compelling power, diversion occurs whenever sensorial stimuli are sufficiently increased in intensity. This child, however, can have no developed associations of any considerable extent around her controlling interests; and yet her absorption in the same is complete, and her attention is incapable of being distracted. There is also a marked difference in her increased ability to hear whenever questioned concerning scenes which she has herself witnessed and in which she has taken part with evident pleasure to herself. For instance, after attending an exhibition of Hagenbeck's animals in New York, she heard and replied to all questions put to her concerning the animals and their performances. In this no doubt there was an ideational reinforcement of the auditory stimuli through the memory pictures still vividly impressed upon her mind. The ideational processes causing motor discharges which in time would increase the intensity of the sensation. This would form a stimulus additional to the mental energy arising from the increased interest already

¹ James, *Psychology*, I, p. 417.

mentioned.¹ In this connection it may be of interest to quote a sentence from Prof. James that bears upon this point: "We see how we can attend to a companion's voice in the midst of noises which pass unnoticed, though objectively much louder than the words we hear. Each word is *doubly* awakened; once from without by the lips of the talker, but already before that from within by the premonitory processes irradiating from the previous words, and by the dim arousal of all processes that are connected with the 'topic' of the talk. The irrelevant noises, on the other hand, are awakened only once. They form an unconnected train."²

In accounting for such a phenomenon, it is well also to take into consideration the physiological conditions which tend to increase the intensity of a sensation when the attention is unusually concentrated upon it. There seem to be indications of increased circulation in the parts concerned. This is stated by Dr. Cappie in his article on 'The Physiology of Attention and Volition'³: "The mental effect produced by an impression on a sensory surface is stronger, and details about the impressing cause are more completely gathered in when the mind is concentrated on it. . . . Two factors, at least, may be specified as bearing on this problem. In the first place, when the consciousness is engrossed by an immediate sensation, the sphere of encephalic activity is comparatively restricted. In the second place, the encephalic circulation will be focused in the direction of activity. The molecular agitation occasions a necessity and an attraction for more blood, and determination of this takes place all the more freely on account of the quiescence of the large part of the brain. The latter has, as it were, loosened its hold on the circulation, and the impetus towards those parts which have an attraction for it is thus all the stronger. The increased activity of the circulation then reacts on the energies of the tissue, and the mental effect produced is therefore greater."

¹ Baldwin, *Mental Development: Methods and Processes*, pp. 462-3.

² James, *Psychology*, Vol. I, p. 450.

³ *Popular Science Monthly*, Vol. XXX, pp. 231-2.

Now, in the case of this child, it seems as though the conditions, both physiological and psychological, are present in exaggerated form, in order to produce the unusual results. The difference, however, between this case and normal instances is a difference rather of degree than of kind.

An additional feature of interest lies in the similarity between the phenomena we have been considering and the phenomena often accompanying attacks of hysteria. In such cases there is generally a very restricted field of attention, and the patient becomes so completely absorbed in some engrossing subject as to appear completely oblivious to all sensorial stimuli whatsoever. For instance, take such a case as cited by Pierre Janet: Lucie, while talking to one person, was insensible to all sounds about her, and could even be touched without being conscious of it. And when Léonie was knitting or writing, you might open or shut the door with a loud noise, speak to her, touch her, etc., without her perceiving it at all. Moreover, there were parts of her body which were so extremely sensitive to touch as to provoke cries of pain and even convulsions; and yet, when preoccupied by work or simple conversation, she could be touched upon the same hyperæsthesia spots, with no indication that she perceived this.¹ This account is very similar to the report which I received in reply to a letter which I wrote to the nurse above mentioned, making further inquiry concerning this child's case, and asking particularly whether she could hear when spoken to from behind, where her attention could not be aroused by any visual stimulus. The following is the answer which I received: "Her ability to hear when interested, in comparison with times when not interested, is very marked. She can hear if you stand behind her and talk very loud, but not very well; and *never* when she has her mind concentrated on another object; for instance, if she is at a window, looking out at something which has her attention, it is impossible to make her hear."

A case somewhat similar, yet with certain interesting peculiarities, is quoted by Pick from Pitres'² *Leçons cliniques*

¹ Pierre Janet, *L'Automatisme Psychologique*, pp. 188-9.

² *Zeit. für Psych.*, p. 168.

sur l'hysterie: "The patient, with eyes open, could hear; but with eyes closed could not; with one eye open it was possible to hear in the ear opposite, but not in the ear upon the same side as the opened eye." In this same article by Pick, *Ueber die sogenannte Conscience Musculaire (Duchenne)*, there is a general historical survey from the time of Duchenne to the present concerning cases of anæsthesia, in which motor activities have been mediated through visual attention, indicating the quickening of motor as well as sensory functions, by means of a concentration of attention. In all cases where there was not this aid of attention through auditory or visual direction, the attempted movements were impossible. It has been also observed that in the somnambulistic state subjects are sensitive to the voice of the hypnotizer, but do not hear other voices. M. Janet mentions the case of one in this condition who could see a candle lighted by himself, but not those lighted by others; and adds that such "is not peculiar to the somnambulistic state, but exists in high degree among all persons susceptible to suggestion. It is an exaggerated state of distraction which is not merely temporary, and not the result of voluntary attention directed exclusively to one sense; but it is a state of natural and perpetual distraction which prevents these persons from perceiving any other sensation than that which actually occupies the whole field of consciousness."¹

The case of this child seems to occupy a position midway between the temporary and permanent states of distraction as mentioned in M. Janet's account. In all of the similar instances which I have given, as parallel to this case, I have endeavored to indicate varying stages of concentrated attention from the normal to the exaggerated and abnormal; and as closely related phenomena we may consider them as different manifestations of one and the same tendency—the intensified force of attention producing an exaggerated modification of the intensity of sensorial stimuli.

¹P. Janet. *L'Automatisme Psychologique*, p. 189.



THE PERCEPTION OF TWO POINTS NOT THE SPACE-THRESHOLD.

BY GUY TAWNEY,

Ex-Fellow Princeton College.

In the older psycho-physical conception of Weber and Fechner, the space-threshold of a locality on the skin is that distance of two stimulating points from each other at which they are first perceived as two. The classical works of Weber, 'De Pulsu, Resorptione, Auditu, et Tactu,' and 'Tastsinn und Gemeingefühl,' first excited physiologists and psychologists to seek an exact knowledge of this distance for different localities on the skin and to form some physiological explanation of its regularities and variations. Fechner, using the terminology of Herbart, first named this distance the *Raumschwelle*, and the term has come to be used to a greater or lesser extent in psychological literature. The conception is mathematical in so far as it is based on the geometrical fact that two points are necessary to the simplest form of space-extension. It is physiological in so far as based upon Weber's theory of sensory circles, according to which two or more 'sensory circles' must lie unstimulated between two 'touched circles' in order that space, in its simplest form, be tactually perceived. The conception presupposes that there is a space-threshold; that it is the point of transition from the sensation of one point to that of two; and that it is to be found either by the so-called 'method of least perceptible changes' or by that 'of right and wrong cases,' provided the answers collected be passed through one or another of the formulas of Fechner, Müller and Camerer, all of which are based upon the Gaussian formula¹ of the theory of Probability.

These three formulas arose in connection with the method of right and wrong cases which Vierordt first formulated and

¹ This formula contains but two variables.

applied.¹ It was found from the first that between the sensation of one point and that of two, a variety of sensations which can neither be classed as those of one point nor of two appear. Of the pupils of Vierordt in the physiological institute at Tübingen, Kottenkamp and Ulrich² divided the sensations which appear in such experiments into the following classes—I. Double sensations, including *a*) those with a correct and *b*) those with an incorrect judgment of the affected spots of skin; II. simple sensations, *c*) pointed or *d*) as if the skin were touched with a long-shaped instrument, *a*) correctly so felt and *β*) incorrectly. Out of these cases they included only I *a*) under the category of 'right judgments,' leaving all the others to the class of 'wrong' ones. Paulus³ and Riecker⁴, as also Schimpf⁵ and Hartmann⁶ adopted the same classification, adding only the answer 'undecided' to the list of 'wrong cases.' In his first series of experiments,⁷ Dr. Camerer subsumed 'all sensations which cannot be nearer described than that they seem to be produced, not by one pin-point, but by something more extensive,' among the cases of 'right judgments.' But in his later series⁸ he accepted the four answers, 'two points,' 'more than one point,' 'undetermined,' and 'one simple point.'

To dispose of these troublesome groups of intermediate sensations, the three mathematical formulas of Fechner, Müller, and Camerer, each claiming superiority to the other two, were constructed. Their purpose is to reduce, by a simple calculation in the Theory of Probability, this numerous group of intermediate answers to the two variables, *r* and *f*, or *r* and *z*, which the formulas contain. In Camerer's first experiments in which the answers were 'one point,' 'two points,' and 'undecided,' that latter group were evenly di-

¹ Unterschieds empfindlichkeit im Schallgebiete—Vierordt's Archiv, 1856, Heft 2, p, 185.

² Versuche über den Raumsinn der Haut der oberen Extremitäten, p. 42.

³ Versuche über den Raumsinn der Haut der oberen Extremitäten, p. 3.

⁴ Versuche über den Raumsinn der Kopfhaut, Tabelle II, p. 14.

⁵ Raumsinn der unteren Extremität bei Anchylose des Kniegelenks I, p. 11 and ff.

⁶ Raumsinn der Haut des Rumpfes und des Halses. Tabelle I, p. 7.

⁷ Versuche über den Raumsinn der Haut nach der Methode der r. u. f. Fälle, I.

⁸ Versuche über den Raumsinn der Haut nach der Methode der r. u. f. Fälle, No. II, p. 285 ff.

vided between the two classes of 'right' and 'wrong judgments.' That all these methods leave much room for gross inaccuracies in results seems admitted by all. Nor do the elaborate formulas settle the question. The discussion of their relative values seems to have died with their champions, and the applicability of the methods of right and wrong cases to the determination of the so-called *Raumschwelle* is still an open question in the school of Psycho-physics, as the late discussions of Merkel amply demonstrate.

A more recent view has offered a somewhat different conception of space according to which it is based upon a quality of sensation as such. According to Külpe this quality, viz., extensity (*Ausgedehntheit*) belongs to sensations of sight and touch¹: according to James² and Ward,³ to all sensations. In connection with a series of experiments to determine the effect of exercise on the perception of two points, it was thought that a new side of the facts in regard to the tactual perception of space might be gained by asking the observer to describe his sensations, as fully as possible, giving their spatial characteristics and, in connection with the perception of two points, their apparent distances apart. A large number of the descriptions received are difficult to classify and cannot be conveniently given in the form of tables; but enough can be thrown into the following groups to convince one that every sensation of touch has a space-quality which at once becomes apparent through the comparison of two or more different sensations with each other.

The observers in these experiments were Herr Max Arrer (Ar.), M. Victor Henri (H.), Rev. S. Gringe Hefelbower (Hef.), and Messrs. G. M. Stratton (St.), A. Müller (A. M.), and G. Tawney (T.). We wish here to express our thanks to these five gentlemen for their indispensable assistance. Table I. gives the cases in which one sensation only was felt. In the first vertical column are the observers; in the following four the applied stimuli, viz., one point, two points whose distance apart is below the threshold for the

¹ Grundriss der Psychologie, p. 347, § 3.

² Principles of Psychology, Vol. II, Chap. XX, p. 135.

³ Encyclopædia Britannica, Article 'Psychology,' pp. 49, 53.

perception of two points, two points near the threshold, two points over the threshold; in the following four columns are given the answers received, thrown into the following groups: 'small,' 'sharp' or 'pointed;' 'medium,' 'round' or 'good;' 'large,' 'blunt' or 'extended,' and 'a line' or 'lengthy sensation.' The adjective 'good' was used by nearly all, and when asked what they meant, they answered 'medium-sized,' 'round,' 'solid,' 'not to be mistaken,' 'easy to recognize,' etc. The instrument used in all the experiments was a simple pair of compasses, into which fine, carefully-prepared bone points had been inserted.

TABLE I.—Descriptions of 667 single sensations in terms of space, the stimuli being 1 point, 2 points below the threshold, 2 about the threshold, and 2 over the threshold.

OBSERVER.	STIMULUS.				ANSWER.			
	One point.	Two points under the threshold.	Two points about threshold.	Two points over threshold.	'Small,' 'sharp' 'pointed.'	'Medium,' 'good' 'spherical.'	'Large,' 'blunt' 'extended.'	'A line' or 'lengthy sensation.'
Ar.	12	84	58	13	2 11	7 30 20 1	1 3 11 3	2 40 27 9
H.	45	27	3		23 6	5 6 1	12 11 2	5 4
St.	26	22	3		7 3	6 3	2 6	11 10 3
A. M.	3	39	30		3 10	2 11	27 19	
Hef.	30	84	77	21	20 7 4	10 17 6 1	8 10 9 2	2 50 58 18
T.	56	22	4	8	15 12	15 2 2 4	17 4 1 3	9 4 1 1

In this table the sensations shift gradually from the first column, 'small and pointed,' toward the last two, as the stimulus passes from one point to two points over the threshold. In the cases of H., A. M., and St., the absence from the table of experiments with two points over the threshold is due to the fact that these observers seldom or never mistook two points over the threshold for one point as the others so often did. The table shows that the space-quality of the sensations of different persons varies widely. Only a very general regularity exists between them. A. M. seemed not to experience single long sensations at all, while St. and Hef. seemed to have more lengthy ones than any other kind. I touched the arm of A. M. with the edge of a visiting-card and asked whether he ever had similar sensations from the compass-points. His answer was an unqualified no.¹

Table II. gives experiments in which two sensations were felt and described. In the first two vertical columns are the observers and the stimuli for each; in the following seven are the judgments, divided into two classes, where the sensations were alike, and where they were unlike or different. In the first class the two points are alike and either 'small' and 'sharp,' 'medium-sized' and 'spherical,' 'large' and 'blunt,' or 'two points with a line connecting them;' in the second class the points are different: 'the one large and the other small,' 'the one lengthy and the other round,' 'different in space-quality, but connected by a line or long sensation.'

¹ It may be significant that the muscles of H. and A. M., those of A. M. especially, were hard and round at the investigated places, filling out the skin so as to prevent its movement; while those of St. are comparatively soft, and those of Hef. rather fleshy, permitting the compass-points of their own weight to sink into them and thus causing comparatively extensive movements of the skin. This may explain the fact of their frequency with St. and Hef. and their infrequency with H. and A. M. In any case the cause of these variations seems to be chiefly peripheral, as distinct from imagination, expectation, etc.

TABLE II.—Descriptions of 1063 double sensations, 765 alike and 298 unlike, the stimuli being 1 point, 2 points below the threshold, 2 points near the threshold, and 2 points above it.

OBSERVER.	STIMULUS.	TWO POINTS FELT ALIKE.				TWO POINTS FELT UNLIKE.		
		'Small, 'pointed,'	'Medium, 'spherical, 'good,'	'Large, 'blunt, 'diffuse,'	'A line, or 'lengthy sensation between.	'One large, the other small,'	'One lengthy, the other round,'	'Two unlike points connected,'
Ar.	One point.					1		
	2 p'ts under threshold.	10	5		1	20	10	5
	2 p'ts about thres.	15	12	5	6	26	16	3
	2 p'ts over thres.	25	20	15	1	2		
St.	One point.					8	5	2
	2 p'ts under thres.	28	25	6	23	11	4	4
	2 p'ts about thres.	31	20	13	30	1	1	1
	2 p'ts over thres.	5	5	6	5	2	1	
A. M.	One point.							
	2 p'ts under thres.	5	1			2		
	2 p'ts about thres.	3				7		
	2 p'ts over thres.	6	7	4	1	3	4	
H.	One point.							
	2 p'ts under thres.	5	22	1		7		
	2 p'ts about thres.	22	22	10	8	1		
	2 p'ts over thres.	7	5	1	2	20	5	10
Ta.	One point.							
	2 p'ts under thres.	4	3	1	5	25	3	16
	2 p'ts about thres.	23	19	7	2	2		
	2 p'ts over thres.	50	21	1	27	38	4	6
	One point.							
	2 p'ts under thres.	7	2		6	1		
	2 p'ts about thres.	4	1		2	2		
	2 p'ts over thres.	61	32	4	15	15		5

Table II. gives ample illustration of the fact which we have in hand, viz., that all sensations of touch have a space-quality. It will be noticed that the larger proportion of the cases where the two points are different are stimulated by one point or by two points under the threshold. For example, Ar. felt two points alike 10 times and unlike 20 times when the stimulating points were below the threshold, 15 times alike and 26 times unlike when the points were near the threshold, but 25 times alike and only 2 times unlike when the points were over the threshold. This fact accords

with the self-observation of Ar. that there is always a difference between the two sensations of a so-called *Vexirfehler*, where two points are felt where only one is touched, such that he can in most cases recognize the illusory and the genuine points. But this was not the observation of St. or A. M., but rather the opposite. In the case of Hef. the two sensations from two points over the threshold were always felt as separate, round, solid, and perfectly alike. But what the cause of these differences in different observers may be we are not able to surmise owing to the lack of a large number of observers. The false perception of two points where only one point was touched was most frequent with St. and T.; and least frequent with Hef. who seems to possess in general a very highly developed and very healthy sensory nervous system.

Variations in the 'threshold' were frequent with the same individuals, not only from day to day, but also within the same hour. One observer was found in Wundt's institute who has taken part in numerous skin-experiments, on the volar side of whose lower arm a 'threshold' could not be found which remained constant for a half hour; a similar experience was that with St. and T. Moreover, we made the attempt to repeat the same experiment several times in succession under exactly the same conditions. An example of the results obtained is the following. The place is the volar side of St.'s right lower arm, as it lay unmoved throughout the experiments on the table. The distance apart of the points was 20 mm. The spots on the skin were the same in each experiment, the time interval being always about two minutes. The pressure in each trial was the same, viz., the weight of the compasses. His answers were as follows:

First experiment—two points, 15 mm. apart, clear, equally strong, simultaneously and immediately perceived.

Second experiment—at first a line; then two distinct ends which became perfect points about 30 mm. apart but connected by a line.

Third experiment—one point, sharp, deep, somewhat painful.

Fourth experiment—two points separated about 20 mm., but lying at right angles to the above two points.

Fifth experiment—one point, somewhat large.

Sixth experiment—at first several points: then three became clearer than the remainder: at last one seemed a real point surrounded by a group of fainter ones.

Seventh experiment—at first two points bound together by a line: then a large lengthy sensation about 15 mm. in length.

Eighth experiment—two points about 12 mm. apart, clear, equally strong and simultaneous.

Ninth experiment—one point, small, simple, and definite.

Tenth experiment—two points, 10 mm. apart, simultaneous, equally strong, becoming painful.

Experiments similar to these were made on H. and, later, by H. on T. with the same general results. Such variations are well known to every observer of skin-sensations. The genius of Fechner did not succeed in reducing their manifoldness to simple regularity. Such experiments seem to show clearly that the perception of two points takes place under conditions too varying and too different to be regarded as the first tactual space-perception. Our tactual sense of space seems to be far more exact and far more regular than the perception of two points.

From these and similar experiments it seems that there is no such thing as a 'space-threshold' in the entire field of skin-sensations, because there is no sensation of touch, not even that of a fine needle-point, which does not already possess a spatial quality. The latter does not enter into sensations of touch at the perception of two points. The mathematical point, a point without extension, does not exist either to sight or touch. Geometrical extension in one direction begins with two points, but tactual extensity-perception clearly begins with the comparison of simple tactual sensations. The difference between a point and a line like the edge of a visiting-card is sooner perceived on the lower arm at least, than the difference between two points, thus showing that the perception of extensity through touch does not depend upon the experience of more than one simple sensation. We are fully convinced that the sensation of one point, however fine, has in it the data for abstracting three dimensions by comparison with other points, *i. e.*, by the usual

process of assimilation and discrimination which underlie all perception. The space-threshold should be a certain moment in sensations where extensity, *i. e.*, spatiality, first enters consciousness; but the *Raumschwelle* of Weber and Fechner is the moment where two simultaneous touches enter consciousness which we have seen comes much later and under much more varying conditions—it is in short not a *Raumschwelle* at all. If we wish to speak of a space-threshold at all, we should designate by the term a fact of assimilation rather than any measurements on the surface of the skin. ‘The fineness of the locality sense’ (*Feinheit des Ortsinnes*) is, properly speaking, the object of all such measurements, but never the ‘space-threshold.’ We have shown that single sensations and double sensations are both indefinitely various, but the variations are not without some regularity corresponding to the outer stimulus. The single point, the line, the surface, and even the solid, are all perceptions of touch which have their origin in the subjective and objective conditions of the sensations. In short, we have here a large field of sensations which has never been exhaustively investigated. Sensations belonging to this field have, until very lately, been regarded as mere hindrances to the ascertainment of the *Raumschwelle*, and have been either ignored, as in the first experiments of Camerer and those of Vierordt’s pupils, or dealt with as food for psychic threshing-machines, such as the formulas of Fechner, Camerer and Müller.

Finally, the conception of a *Raumschwelle* is nothing more than a remnant of the old way, ‘von oben nach unten,’ of ‘Scholastic deduction,’ which Fechner strove so faithfully to eradicate from psychology. It is the carrying downward, ‘von oben nach unten,’ of a physiological and mathematical conception—a reading into sensations of the forms of a highly abstract intellect; whereas the mathematical conception is in fact an abstract of the spatial quality of sensations themselves. It may be that when psychologists have studied sensations humbly and exhaustively they will find among them, and in all of them, the germs of every flower that blows—of both the form and substance of thought, feeling, and will.

THE ORIGIN OF A 'THING' AND ITS NATURE.¹

BY PROFESSOR J. MARK BALDWIN,

Princeton University.

The present growing interest in genetic problems, as well as the current expectation that these discussions may render it necessary that certain great beliefs of our time be overhauled—these things make it important that a clear view should be reached of the sphere of inquiry in which questions of origin may legitimately be asked, and also just what bearing their answer is to have upon the results of the analytic study of philosophy.

We already have, in several recent publications, the inquiry opened under the terms 'origin vs. reality'—or, in an expression a little more sharp in its epistemological meaning, 'origin vs. validity.' I should prefer, in the kind of inquiry taken up in this paper, to give a wider form to the antithesis marked out, and to say 'origin vs. nature': meaning to ask a series of questions all of which may be brought under the general distinction between the 'how' of the question: how a thing arose or came to be what it is; and the 'what' of the question: what a thing is.

Well, first, as to 'what.' Let us see if any answer to the question 'what is it?' can be reached, adequate to our needs, in any case of genetic inquiry. It seems that the philosophy of to-day is pretty well agreed to start analysis of a thing inside of the behavior of the thing. A 'thing' is first of all so much observed behavior. Idealists pass quickly over the behavior, it is true; it is too concrete, too single, for them: it is not to them a thing, but a 'mere thing.' But yet they do not any longer allow this 'mereness' to offend them to the extent of drawing them off to other fields of exploration

¹ Notes presented to the Princeton Psychological Seminar in May, 1895, slightly revised.

altogether. They try to overcome the 'mereness' by making it an incident of a larger fullness: and the 'implications' of the thing, the 'meaning' of it 'in a system'—this shows up the mereness, both in its own insignificance and in its fruitful connection with what is universal.

So we may safely say of the idealist, that if he get a doctrine of a 'thing,' it must, he will himself admit, not be of such a thing that it cannot take on the particular form of behavior which the one 'mere thing' under examination is showing at the moment. There must, in short, be no contradiction between the 'real thing' and the special instance of it which is found in the 'mere thing.'

He, the idealist, therefore, is first of all a phenomenist in getting his doctrine of the real; the 'what' must be, when empirically considered, in some way an outburst of behavior.

Now the idealist is the only man, I think, of whom there is any doubt in the matter of this doctrine of behavior, except the natural realist, who comes up later. Others hold it as a postulate since Lotze, and later Bradley, did so conclusively show the absurdity of the older uncritical view which held, in some form or another, what I may call the 'lump' theory of reality. A thing can not be simply a lump. Even in matter—so we are now taught by the physicists—there are no lumps. To make a thing a lump—not to cite other objections to it—would be to make it impossible that we should know it as a thing. So all those doctrines which I have classed as other than idealistic accept, and have an interest in defending, the view that the reality of a thing is presented in its behavior.

So setting that down as the first answer to the 'what' question, we may profitably expand it a little. The more we know of behavior of a certain kind, then the more we know of reality, or of the reality, at least, which that kind of behavior is. And it is evident that we may know more of behavior in two ways. We may know more of behavior because we take in more of it at once; this depends on the basis of knowledge we already have—the relative advance of science in description, explanation, etc., upon which our interpretation of the behavior before us rests. In the behavior of a

bird which flits before him, a child sees only a bright object in motion; that is the 'thing' to him. But when the bird flits before a naturalist, he sees a thing whose behavior exhausts about all that is known of the natural sciences. Yet in the two cases there is the 'thing,' in just about the same sense.

When we come, farther, to approach a new thing, we endeavor, in order to know what it is, to find out what it is doing, or what it can do in any artificial circumstances which we may devise. Just as far as it does nothing, or as far as we are unable to get it to do anything, just so far we confess ignorance of what it is. We can neither summon to the understanding of it what we have found out about the behaviour of other things, nor can we make a new class of realities or things to put it in. All analysis is just the finding out of the different centers of behavior which a whole given outburst includes. And the whole, if unanalyzable to any degree, is itself a thing, rather than a collection of things.

But the second aspect of a thing's reality is just as important. Behavior means in some way change. Our lump would remain a lump, and never become a thing, if, to adhere to our phenomenal way of speaking, it did not pass through a series of changes. A thing must have a career; and the length of its career is of immediate interest. We get to know the thing not only by the amount of its behavior, secured by examining a cross-section, so to speak, but also by the increase in the number of these sections which we are able to secure. The successive stages of behavior are necessary in order really to see what the behavior is. This fact underlies the whole series of determinations which ordinarily characterize things, such as cause, change, growth, development, etc., as comes out farther below.

The strict adherence to the definition of a thing in terms of behavior, therefore, would seem to require that we waited for the changes in any case to go through a part at least of their progress—for the career to be unrolled, that is, at least in part. Immediate description gives, as far as it is truly immediate, no science, no real thing with any richness of content; it gives merely the snap-object of the child. And

if this is true of science, of every-day knowledge of things, to live by, how much more of the complete knowledge of things desiderated by philosophy? It would be an interesting task to show that each general aspect of the 'what' in nature has arisen upon just such an interpretation of the salient aspects presented in the career of individual things in nature. But this would be to write a large and most difficult chapter of genetic philosophy.

Our second point in regard to the 'what,' therefore, is that any 'what' whatever is in large measure made up of judgments based upon experiences of the 'how.' The fundamental concepts of philosophy reflect these categories of origin, both in their application to individuals—to the 'mere thing'—and also in the interpretation which they have a right to claim: for they are our mental ways of dealing with what is 'mere' on one hand and of the final reading of reality which philosophy makes its method. Of course the question may be asked: How far, origin? That is, how far back in the career of the thing it is necessary to go to call the halting-place 'origin.' This we may well return to lower down; the point here is that origin is always a reading of part of the very career which is the content of the concept of the nature of the thing.

Coming now closer to particular instances of the 'what,' and selecting the most refractory case that there is in the world, let us ask these questions concerning the *mind*. I select this case because, in the first place, it is the case urgently pressing upon us; and, second, because it is the case in which there seems to be, if anywhere, a gaping distinction between the 'what' and the 'how.' Modern evolution claims to discuss the 'how' only, not to concern itself with the 'what;' or, again, it claims to solve the 'what' entirely by its theory of the 'how.' To these claims what shall we say?

From our preceding remarks it seems evident that the nature of mind is its behavior generalized; and, further, that this generalization necessarily implicates more or less of the history of mind; that is, more or less of the career which

discloses the 'how' of mind. What further can be said of it as a particular instance of reality?

A most striking fact comes up immediately, when we begin to consider mental and with it biological reality. The fact of growth, or to put the fact on its widest footing, the fact of *organization*. The changes in the external world which constitute the career of a thing, and so show forth its claim to be considered a thing, fall under some very wide generalizations, such as those of chemistry, mechanics, etc.; and when the examination of the thing's behavior has secured its description under these principles in a pretty exhaustive way, we say the thing is understood. But the things of life, and the series of so-called organic changes which unroll its career, are not yet so broadly statable. When we come to mind, again, we find certain pretty well made out generalizations of its behavior. But here, as in the case of life, the men who know most have not a shadow of the complacency with which the physicist and the chemist categorize their material. It is for this reason, I think, in part, that the difference between the two cases gets its emphasis, and the antithesis between origin and nature seems so necessary in one case while it is never raised in the other. For who ever heard a natural science man say that the resolution of a chemical compound into its elements, thus demonstrating the elements and law of the origin of the 'thing' analyzed, did not solve the question of its nature, as far as science can state a solution of that question?

But we can not say that the whole difference is one of greater modesty on the part of the psychologists. The facts rather account for their modesty. And the prime fact is one formulated in more or less obscurity by many men, beginning with Aristotle: the fact, namely, that organization considered as itself a category of reality never reaches universal statement in experience. To confine the case at first to vital phenomena, we may say that to subsume a plant or animal under the category of organization is to make it at once to a degree an X: a form of reality which, by right of this very subsumption, predicts for itself a phase of behavior as yet unaccomplished—gives a prophecy of more career, as a fact,

but gives no prophecy (apart from other information which we may have) of the new phase of career in kind. Every vital organization has part of its career yet to run. If it has no more career yet to run, it is no longer an organization: it is then dead. It then gets its reality exhausted by the predication of the categories of chemistry, mechanics, etc., which construe all careers retrospectively. A factor of the biological and mental categories alike is just this element of what I have called elsewhere 'Prospective Reference.'¹ In biology it is the fact of 'Accommodation:' in psychology it is the same fact found in all cases of Selection—most acute in Volition.

And it does not matter how the content in any particular filling up of the category may be construed after it takes on the form of accomplished fact—after, *i. e.*, it becomes a matter of 'retrospect.' All constructions in terms of content mean the substitution of the retrospective categories for those of prospect; that is, the construction of an organization after it is dead, or—what amounts to the same thing—by analogy with other organizations which have run down, or died, in our experience. Suppose, for example, we take the construction of the category of Accommodation, in each particular instance of it, in terms of the ordinary biological law of natural selection—an attempt made by the present writer under the statement of so-called 'Organic Selection'²—and so get a statement of how an organism actually got any one of the special adaptations of its mature personal life. What, then, have we done? I think it is evident that we have simply resorted to the 'retrospective' reference; we have changed our category in the attempt to get a concrete filling for a particular case *after it has happened*. To adopt the view that the category of organization can be in every case filled up with matter, in this way, does not in any sense destroy the prospective element in the category of organization; for the psychological subtlety still remains in mind in the

¹See my *Mental Development: Methods and Processes* (Macmillan & Co.), Chaps. VII, XI.

²In accordance with which the organism's new accommodations are selected out of movements excessively produced under pleasure-pain stimulation. *Ibid.* pp. 174 f.

doing of it, either that the event must be awaited to determine the outcome, and that I am agreeing with myself and my scientific friends to wait for it, or that we are solving this case by others for which we did wait. A good instance of our mental subtleties in such cases is seen in the category of 'potentiality,' considered lower down. The extreme case of the reduction of the categories of prospective reference to those of retrospect, is evidently the formula for probabilities. I do not see how that formula can escape being considered a category of retrospect, applied to material which does not admit of any narrower or more special retrospective formulation.

Now the inference from this is that our predicate 'reality,' in certain cases, is not adequately expressed in terms of the experienced behavior of so-called real content. The very experience on the basis of which we are wont to predicate reality testifies to its own inadequacy. I see no way to avoid the alternatives that either the notion of reality does not rest upon experiences of behavior, or that the problematic judgments based upon those experiences of progressive organization which we know currently under the term development, are as fundamental to these kinds of reality as are those more static judgments based on history or origin.

It may be well, in view of the importance of this conclusion, to see something more of its bearings in philosophy. The historical theories of design, or teleology in nature, have involved this question. And those familiar with the details of the design arguments pro and con will not need to have brought to mind the confusion which has arisen from the mixing up of the 'prospective' and 'retrospective' points of view. Design, to the mind of many of the older theistic writers, was based upon relative unpredictability—or better, infinite improbability. Such an argument looks forward: it is reasoning in the category of organization, but under the 'prospective' reference. The organization called mental must be appealed to. What, was asked, is the probability of the letters of the Iliad falling together so as to read out the Iliad? The opponents, on the other hand, have said: Why is not the Iliad combination as natural as any other?

One combination, has to happen; what is to prevent this? If a child who cannot read should throw the letters, the Iliad combination is no more strange to him than any other. These men are reasoning in the retrospective categories. They are interpreting facts. The fault of the latter position is that it fails to see in reality the element of higher organization which the whole series when looked at from the point of view of the real Iliad requires. What would really happen, if the child should throw the Iliad combination, would be that nature had produced a second time a combination once before produced (in the mind of Homer, and through him in ours) without fulfilling all the other combinations—an infinite number—which have a right to be fulfilled before the Iliad combination be reproduced. But this added element of organization needed to bring nature into accord with thought and which the postulate of design makes in reaching a Designer—this is not needed from the mere historical or retrospective examination of the facts. In other words, if the opponents of design are right in holding to a complete reduction of organization to retrospective categories, they ought to be able to say just as definitely that the Iliad combination will happen in a certain number of throws, as they are to say afterwards that it has happened.

The later arguments for design, therefore, which tend to identify it with organization, and to see in it, so far as it differs from natural law, simply a harking forward to that career of things which is not yet unrolled, but which when completely unrolled will be a part of the final statement of origins in terms of natural law—this general view has the justification of as much criticism as has now been stated.

And, further, it is clear that the two opposed views of adaptation in nature are both genetic views—instead of being, as is sometimes thought, one genetic (that view which interprets the adaptation after it has occurred), and the other analytic or intuitive (that view which seeks a beforehand construction of design). The former of these is usually accredited to the evolution theory; and properly so, seeing that the evolutionist constantly looks backward. But the other view, the design view, is equally genetic. For

the category of higher or mental organization by which it proceeds is just as distinctly an outcome of the movement or drift of experience toward an interpretation of career in terms of history. Teleology, then, when brought to its stronghold, is a genetic outcome, and owes what force it has to the very point of view that its most fervent advocates—especially its theological advocates—are in the habit of running down. The consideration of the stream of genetic history itself, no less than the attempt to explain the progress of the world as a whole, its career, leads us to admit that the real need of thinking of the future in terms of organization is as great as the need of thinking of the past in terms of natural law. The need of so-called mental organization or design is found in the inadequacy of natural law to explain the further career of the world, and its past career also, as soon as we go back to any place in the past and ask the same question there. It would be possible, also, to take up the last remark for more thought, and to make out a case for the proposition that the categories of 'retrospective' thinking also involve a strain of organization—a proposition which is equivalent to one which the idealists are forcibly urging from other grounds and from another point of view. Lotze's argument to an organization at the bottom of natural causation has lost nothing of its power. Viewed as a category of experience, I am unable to see the force of the assumption tacitly made by the Positivists, and as tacitly admitted by their antagonists, that causation is to be ultimately viewed entirely under such retrospective constructions as 'conservation of energy,' etc. Such constructions involve an endless retrospective series. And that is to say that the problem of origin is finally insoluble. Well, so it may be. But yet one may ask why this emphasis of the 'retrospective,' which has arisen in experience with just the basis of experience that the 'prospective' also has? It may be a matter of taste; it may be a matter of 'original sin.' But if we go on to try to unite our categories of experience in some kind of a broader logical category, the notion of the Ultimate must, it would seem, require both of the aspects which our conception of reality includes; the 'prospective' no less than the 'retro-

spective.' Origins must take place continually as truly as must sufficient reasons. The only way to avoid this is to say that reality has neither forward nor backward reference. So say the idealists in getting thought which is not in time. But be that as it may, we are dealing with experience—though for myself, I must say, thought which looks neither backward nor forward is no thought at all.

Another subtlety might raise its head in the inquiry whether in their origin all the categories did not have their 'natural history.' If so, it might be said, we are bound, in the very fact of thinking at all, to give exclusive recognition to the historical aspect of reality. But here is just the question: does the outcome of career to date give exhaustive statement of the idea of the career as a whole? There would seem to be two valid objections to it. First, it would be, even from the strictly objective point of view, the point of view of physical science, to construe the thing mind entirely in terms of the behavior of its stages antecedent to the present: that is entirely in terms of descriptive content, by use of the categories of retrospective interpretation. And, second, it does not follow that because a mental way of regarding the world is itself a genetic growth, therefore it is an illusory way. Let me explain these two points a little.

1. A chemist seemed justified in looking at atmospheric air, as explained by the formula for a mixture of nitrogen and hydrogen, for the reason, and this is his practical test, that the behavior of air confirms that view. His confidence in his statements of history can only be justified on the ground that present history never contradicts it. But as soon as a new experiment showed that new behavior may be different, and may contradict the reports of history, he looks for a new thing, argon—new in the sense, of course, that the historical manifestations of the kind of reality in so-called air had never before brought it to recognition. In other words, the nature of air had been stated in terms of oxygen and nitrogen; but he now sees that the statement founded on what was known of origin—and that is what origin means in all these discussions—was inadequate. This would seem to admit, however, that if the problem of origin could be really exhausted,

that of nature would be exhausted too; and no doubt it would. But it is a corollary from the second point of objection, soon to be made, that the problem of origin can never be exhausted, even by philosophy, without an appeal to other than the historical or retrospective categories.

But before I pass on to the second objection to the position that a thing which is admitted to have had a natural history must have its interpretation adequately given in that history, and that this applies also to the very categories by the use of which its denial is effected—before going farther I may point out an extreme case of the main position as sometimes argued by evolutionists. If, it may be said, the mind has developed under constant stimulations from the external world, and if its progress consists essentially in the more and more adequate representation in consciousness of the relations already existing in the external world, then it follows that these internal representations can never do more than reflect the historical events of experience. Consciousness simply testifies again to the real as it has been testified to her before. How, then, can there be any such thing as a phase of reality not subject to plain statement under natural law?¹

This a very common objection to all thorough-going statements of mental evolution. It rests on the mistaken view, just pointed out, that a statement of the historical career of a thing can ever be an adequate statement of its nature; in other words, that the origin of the categories of thought can tell what these categories will do—what their function and meaning is in the general movement of reality. Consciousness is entitled to a hearing in terms of its behavior solely. Its behavior, attitudes, etc., represented by

¹ It is this supposed necessity that leads Mr. Huxley to hold that evolution cannot explain ethics, *i. e.*, the supposed necessity that the validity of ethical values must be adequately found in the terms of their origin; for, says he, the pursuit of evil would have as much sanction as that of good, for both are in us, and they would have the same origin (*Evol. and Ethics*, esp. p. 31). But to say, as we do, that the appeal made by the word 'ought' is a 'prospective' appeal, as opposed to the description of the 'is,' which is 'retrospective,' does not require us to say that the impulse to recognize either is not a product of evolution. My discussion of Prof. Royce's attempt (*Int. Jour. of Eth.*, July, 1895) to show the psychological origin of the antithesis between 'ought' and 'is,' may be referred to (*Int. Jour. of Eth.*, Oct., 1895).

'prospective' thought are there just as its behavior represented by its history is there. Who would venture to say that consciousness of a relation in nature is in no sense a different mode of behavior from the relation itself in nature? The real point is in what I have already tried to put in evidence: that such a construction involves the assumption that reality in its movement defines all her own changes in advance of their actual happening. The very series of changes which constitute the basis in experience for the growth in consciousness of the category of change are the basis also for the new aspects of reality (say consciousness) which are held to be only a putting in evidence of the relations already existing in nature. If consciousness is no new thing—on our behavior-definition of thing—then knowledge of the historical movement of reality must be not at all different from the movement which has led up to knowledge. The discovery of the principle of evolution, for example, is not a new event added to the fact that the series evolving was there to be discovered!

But I may be even more concrete. I have recently developed a view of mental development which not only makes each stage of it a matter of legitimate natural history, but goes on to say that the one method of motor adaptation is by imitation. What could be a more inviting field for the criticism: imitation is mere repetition. How can anything new come out of imitation? Not only is consciousness merely repeating the relationships already there in nature, but the development of consciousness itself is merely a series of repetitions of its own acts. I have had this criticism already; especially with reference to volition. How, it is asked, can anything new be willed if volition is in its origin only imitation become complex?

I reply in a way to make concrete what has been said immediately above. The counter question may be put: why can not anything new come out of imitations? Why may not the very repetition be the new thing, or the condition of it? To say not is to say that by looking at the former instance, the historical, after its occurrence, you can say that that occurrence fully expressed mental behavior. On the

contrary, the prospective reference gained by the imitation may bring out something new; the repetition may be just what is needed to bring an important stage in the career of mental reality. In itself, of course, an imitation is no more open to the objection we are considering than any other kind of mental behavior; but it seems to be more so, because it emphasizes the very point that the current objection to natural history hits upon, *i. e.*, that it makes the mind only a means of reinstatement of relations already existing in nature, and then makes that the explicit method of mental history.

2. The second answer to the view now being criticised may be put in some such way as this. It does not follow that because a product—one of the categories of organization, such as design, the ethical, &c.—is itself a matter of gradual growth, its application to reality is in any way invalidated. A category must be complete, ready-made, universal, without exceptions, we are told, in order that its application to particular instances be justified. But I fail to see the peculiar and mysterious validity supposed to attach to an intuition because whenever we think by it we allow no exceptions. Modern critiques of belief and modern theories of nervous habit have given us reasons enough for discarding such touch-stones as 'universality' and 'necessity.' And modern investigations into the race development of beliefs have told us how much better an aspect of reality really is because at one time people insisted in thinking in a certain intuitive way about it. The whole trouble, as I think, with the intuitional way of thinking is curiously enough that fallacy which I have pointed out as being a favorite one of the evolutionists. The evolutionists say that an intuition is of no value when construed prospectively, *i. e.*, as applying to what 'must be' beyond 'what is': it gets all its content, and all its force, from experience. Therefore, all reality is to be construed retrospectively, and no 'thing' is possible except as accounted for as an evolution from historical elements. True after things have happened, it nevertheless fails by thinking career all finished. Why may not experience produce in us a category whose meaning is prophetic? On the other hand, here come the intuitionists and oppose the evo-

lutionists in this way. They say: no thing is possible except as in some way evidenced for. The intuitions are universal and necessary. As such their evidence can not be found in experience. To admit that they had developed would be to admit that their evidence could be found in experience. Consequently they carry their own evidence and their own witness is all the evidence they have. The fallacy again is just the assumption that reality is finished, that categories of retrospective reference exhaust the case. That the series of events which are sufficient ground for the origin of the category should also be sufficient evidence of its validity. That there is a sharp contradiction, therefore, between a doctrine of derivation from experience (which is inadequate as evidence) and application beyond experience. But when we come to see that the categories of prospective thought are equally entitled to application with those of retrospect, we destroy the weapon of evolution to hurt the validity of mental utterances, and at the same time knock out the props upon which the intuitionist has rested his case.

The case stands with mental facts, to sum up, just about as it does with all other facts. An event in nature stays what it is until it changes. So with an event or a belief or any other thing in the mind of the race. It stays what it is until it has to change. Its change, however, is just as much an element in reality as lack of change is; and the weakening of a belief like any other change is the introduction of new phases of reality. A doctrine which holds to intuitions which admit of no prospective exceptions, no novelties, seems to me to commit suicide by handing the whole case over to a mechanical philosophy; for it admits that all validity whatever must be cut from cloth woven out of the historical and descriptive sequences of the mind's origin.

Our conclusions so far may be summed up tentatively in certain propositions as follows:

1. All statements of the nature of a 'thing' get their matter mainly from the processes which they have been known to pass through: that is, statements of nature are for the most part statements of origin.
2. The statements of origin, however, never exhaust the

reality of a thing; since such statements cannot be true to the experiences which they state unless they construe the reality not only as a thing which has had a career but also as one which is about to have a career: for the expectation of the future career rests upon the same historical series as the belief in the past career.

3. All attempts to rule out prospective organization or teleology from the world would be fatal to natural science, which has arisen by provisional interpretations of just this kind of organization: and also to the historical interpretation of the world found in the evolution hypothesis; for the category of teleology is but the prospective reading of the same series which, when read retrospectively, we call evolution.

4. The fact of natural history of any thing, and more especially of mental products, ideas, intuitions, &c., is no argument against its validity or worth as having application beyond the details of its own history; since, if so, then a natural history series can produce nothing new. But that is to deny the existence of the fact or idea itself, for it is a new thing in the series in which it arises.

5. All these points may be held together in a view which gives each mental content a two-fold value in the active life. Each such content begets two attitudes by its function as a genetic factor in the progressive development of the individual. As far as it fulfils earlier habits it begets and confirms the historical or retrospective attitude, as far as it is not entirely exhausted in the channels of habit, so far it begets the expectant or prospective attitude.

There are one or two points among many suggested by the foregoing which it may be well to refer to—selected because uppermost in my own mind. It will be remembered that in speaking of the categories of organization as having prospective reference, I adduced instances largely drawn from the phenomena of life and mind, contrasting them somewhat strongly with those of chemistry, physics, &c. The use afterwards made of these categories now warrants us in turning upon that distinction, in order to see whether our main results hold for the aspects of reality with which these sciences deal as well. I have intimated above in passing that

the other categories of reality, such as causation, mechanism, are really capable of a similar evaluation as that given to teleology. This possibility may be put in a little stronger light.

It is evident, when we come to think of it, that all organization in the world must rest ultimately on the same basis; and the recognition of this is the strength of thorough-going naturalism and absolute idealism alike. The justification of the view is to be made out, it seems to me, by detailed investigation of the genetic development of the categories. The way the child reaches his notion of causation, for example, or that of personality, is evidence of the way we are to consider the great corresponding race-categories of thought to have been reached: and the category of causation is, equally with that of personality, or that of design, a category of organization. The reason that causation is considered a cast-iron thing, implicit in nature in the form of 'conservation of energy,' &c., is that in the growth of the rubrics of thought certain great differentiations have been made in experience according to observed aspects of behavior; and those events which exhibited the more definite, invariable aspects of behavior have been put aside by themselves; not of course by a conscious convention of man's, but by the conventions of the organism working under the very method which we come—when we make it consciously conventional—to call this very category of organization. What is conservation but a kind of organization looked at retrospectively and conventionally? Does it not hold simply because my organism has made the convention that only that class of experiences which are 'objective' and regular and habitual to me shall be treated together, and so shall give rise to such a regular mental construction on my part?

But the tendency to make all experience liable to this kind of causation is an attempt to undo nature's convention—to accept one of her results, which exists only in view of a certain differentiation of the aspects of reality, and apply this universally, to the subversion of the very differentiation on the basis of which it has arisen. The fact that there is a class of experiences whose behavior issues in such a purely

historical statement and arouses in me such a purely habitual attitude, is itself witness to a larger organization—that of the richer consciousness of expectation, volition, prophecy. Otherwise conservation could never have got for itself abstract statement in thought.

The reason that the category of causation has assumed its show of importance, is just that which intuitionist thinkers urge; and another historical example of confusion due to their use of it may be used for illustration. Causation is about as universal a thing—in its application to certain aspects of reality—as could be desired. And we find the men of this school using this fact to reach a certain statement of theism. But they then find a category of 'freedom' claiming the dignity of an intuition also; and although this comes directly in conflict with the uniformity ascribed to the other, nevertheless it also is used to support the same theistic conclusion. The two arguments read: (1) an intelligent God exists because the intelligence in the world must have an adequate cause, and (2) an intelligent God exists because the consciousness of freedom is sufficient evidence of a self-active principle in the world, which is not caused. All we have to say, in order to avoid the difficulty, is that any mental fact is an 'intuition' in reference only to its own content of experience. Intelligence viewed as a natural fact, *i. e.*, retrospectively, has a cause: but freedom in its meaning in reality, *i. e.*, with its prospective outlook, is prophetic of novelties—is not adequately construed in terms of history. So both can be held to be valid, but only by denying universality to both 'intuitions' and confining each to its sphere and peculiar reference in the make up of reality.

Another thing to be referred to in this rough discussion concerns the more precise definition of 'origin.' How much of a thing's career belongs to its origin? How far back must we go to come to origin?

Up to this point I have used the word with a meaning which is very wide. Without trying to find a division of a thing's behavior into the present of it as distinguished from its history; I have rather distinguished the two attitudes of mind engendered by the contemplation of a thing, *i. e.*, the

'retrospective' attitude and the 'prospective' attitude. When we come to ask for any real division between origin and present existence we have to ask what a thing's present value is. In answer to that we have to say that its present value resides very largely in what we expect it to do; and then it occurs to us that what we expect it to do is no more or less than what it has done. So our idea of what is, as was said above, gets its content from what has been. But that is to enquire into its history, or to ask for a fuller or less full statement of its origin or career. So the question before us seems to resolve itself into the task of finding somewhere in a thing's history a line which divides its career up to the present into two parts; one properly described as origin, and the other not. Now, on the view of the naturalist pure and simple there can be no such line. For the attempt to construe a thing entirely in terms of history, entirely in the retrospective categories, would make it impossible for him to stop at any point and say 'this far back is nature and farther back is origin'; for at that point the question might be asked of him 'what is the content of the career which describe the thing's origin?'—and he would have to reply in exactly the same way that he did if we asked him the same question regarding the thing's nature at that point. He would have to say that the origin of the thing observed later was described by career up to that point; and is not that exactly the reply he would give if we asked him what the thing was which then was? So to get any reply to the question of the origin of one thing different from that to the question of the nature of an earlier thing, he would have to go still farther back. But this would only repeat his difficulty. So he would never be able to distinguish between origin and nature except as different terms for describing different sections of one continuous series of aspects of behavior. This dilemma holds also, I think, in the case of the intuitionist. For as far as he denies the natural history view of origins and so escapes the development above he holds to special creation by an intelligent Deity; but to get content to his thought of Deity he resorts to what he knows of mental behavior. The nature of mind then supplies the thought of the origin of mind.

To those who do not shut themselves up, however, to the construction of things in the categories of realized fact, of history, of 'retrospect,' the question of origin is a fruitful one apart from the statement of nature. For at any stage in the career of a thing the two methods of thought are equally applicable. When we ask how a thing originated, we transport ourselves back to a point in its career at which the 'prospective' categories got a filling not *at that stage* already expressed in the content of history. The overplus of behavior is said to have its origin then, even though afterwards the outcome be statable in the categories of retrospect which have *then been widened by this event*. For example, volition originates in the child at the point of its life at which certain conscious experiences issue out of old content, experiences which were not previously there, to the child, in whatever complications of content were there. But once arisen, the experience can be construed as a continuation of the series of events which make up mental history. To the Positivist and to the Intuitionist a sensational account of the genesis of volition, and to the intellectual Idealist an ideological account of it, rule volition out of reality just by the fallacy of thinking exclusively in retrospect; but the truth is to say "granted either account of its origin, it leaves philosophy still to construe it: for if we estimate volition from facts true before volition arose, the sources do not fully describe it; and if we wait to view it after it arises, then the full statement of career must include the widened aspects of behavior which the facts of volition afford."¹

It is interesting also to note, as another case of application of this general distinction between the mental habits represented respectively by the terms 'prospective' and 'retrospective,' that it gives us some suggestions concerning the very obscure concept called potency or potentiality. This *soi-disant* concept or notion has been used by almost every conceivable shade of thought as the repository of that which is unexplained. Aristotle started the pursuit of this

¹ In the PSYCHOLOGICAL REVIEW for Sept., 1895, I criticised Professor Watson's view that the Absolute can be exhausted by our thought, *i. e.*, can be adequately expressed in terms of the organizations of content already effected.

notion and used it in a way which shed much light, it is true, upon the questions of philosophy concerned with change and organization; but his failure to give any analysis of the concept itself has been an example ever since to lesser men. It is astonishing that, with all the metaphysics of causation which the history of philosophy shows, there has been—that is to my knowledge—no thorough-going attempt to trace the psychological meaning of this category. How common it is to hear the expression, ‘this thing exists, not actually, but potentially,’ given as the end of debate, and accepted, too, as the end. I do not care to go now into a historical note on the doctrine of potentiality; it would be indeed mainly an exposition of a chapter of Aristotle’s metaphysics with the refinements on Aristotle due to the logic of the schoolmen and the dogmatic of modern theology. It may suffice to say something of the natural history of the distinction between potential and real existence in the light of the positions now taken.

In brief, then, there are two aspects as we have seen under which reality must in all cases be viewed—the prospective and the retrospective. The retrospective, as has been said, is the summing up of the history which gives positive content to the notion of a thing considered as accomplished career. This aspect, it seems clear, is what is in view when we speak of real existence in contrast with potential existence. It is not indeed adequately rendered by the content supplied by retrospect, since the fact that the two predicates are held in mind together as both together applicable to any concrete developing thing, forbids us to construe real existence altogether apart from the fact that it has a farther issue in farther career. It is a great merit of Aristotle that he forbade just this attempt to consider the *dunamis* apart from the *energeia*. But, nevertheless, it is true psychologically that real existence is exhausted as a content-predicate with the backward aspect of the series of changes which give body to reality.

And it seems equally evident at first blush that potential existence is equally concerned with the prospective reference of the thought of things. That this is so is perhaps the one element in the notion of potency that all who use the word

would agree upon. But this is inadequate as a description of the category of potentiality. For if that were all, how would it differ from any other thought of the prospective? We may think of the future career of a thing simple in terms of time; that, we would probably agree, does not involve potentiality. A particular potency is confined to a particular thing, *i. e.*, to a particular series of events making up a more or less isolated career. If only the bare fact of futurity were involved, why should not any new unrolling of career be the potency of any thing indiscriminately?

This leads us to see that potency or potentiality, even when used in the abstract, is never free from its concrete reference. And this concrete reference is not that of conception in general, only or mainly; the concrete reference of conception generally is a matter of retrospect, *i. e.*, of the application of the concept to individual things, as far as such application has been justified by historical instances. Indeed, it is the very occurrence of the historical instances which has given rise to the concept, and it generalizes them.

So when we put ourselves at the point of view of the concrete, we have to ask what is actually meant by us when we say a thing exists potentially, over and above the mere meaning that the thing is to exist in the future. We have seen that one added element of meaning is that the thing which is to exist in the future is in some way tied down in its manifestations to something that already exists actually; it must be the potentiality of some one thing in order to be a potentiality at all. Now, how can this be?

Of course the ordinary answer is at once on our lips: the answer that the bond between the thing that is and the thing that is to be is the bond of causation. The potentiality is the unexpressed causal efficacy of the thing that is. But when we come to ask what this means, we find that we are hiding behind one of the screens of common sense. The very fact of cause, whatever bond it may represent from an ontological point of view, is at least a fact of career. The effect is a further statement of the career of the thing called the cause. Now, to say that the potency of a thing is its unexpressed causal power, is only to say that the thing has

not finished its career, and that is a part of the general notion of a thing. That fact alone does not in any way define the future career for us, except in the way of repetition of past career. We merely expect the thing to do what it has done before; not to become some new thing out of the old. In short, the category of causation is not adequate, since it construes all career retrospectively.

We have, therefore, two positions so far, saying (1) that every potency is the potency of a thing, and this means that it gets its content in some way from the historical series which that thing embodies; but (2) that it is something more than a restatement of any or all of the elements of the series thus embodied. Now, what else is there?

The remaining element in the category of potentiality involves, I think, a very subtle movement of the mind along the same distinction of the prospective from the retrospective. Briefly, the potentiality which I ascribe to a thing is my general expectation of more career in reference to it, with the added sense, based on the combined experiences of mine that the prospective does get a retrospective filling after it has happened, that the new career of the thing to which I ascribe the potency, although not yet unfolded, will likewise be capable of retrospective interpretation as further statement of the one series which now defines the thing.

In short, there are three elements or phases of consciousness in this matter: first, let us say, the general prospective element, the expectation that something will happen; second, the causation or retrospective element, the expectation that when it has happened it will be a consistent part of the history of the thing; and, third, the conscious setting back of my observation to the dividing line between these two points of view, and the contemplation of the thing under both of them—both as a present thing, and as a thing for what it will be when the future becomes present.

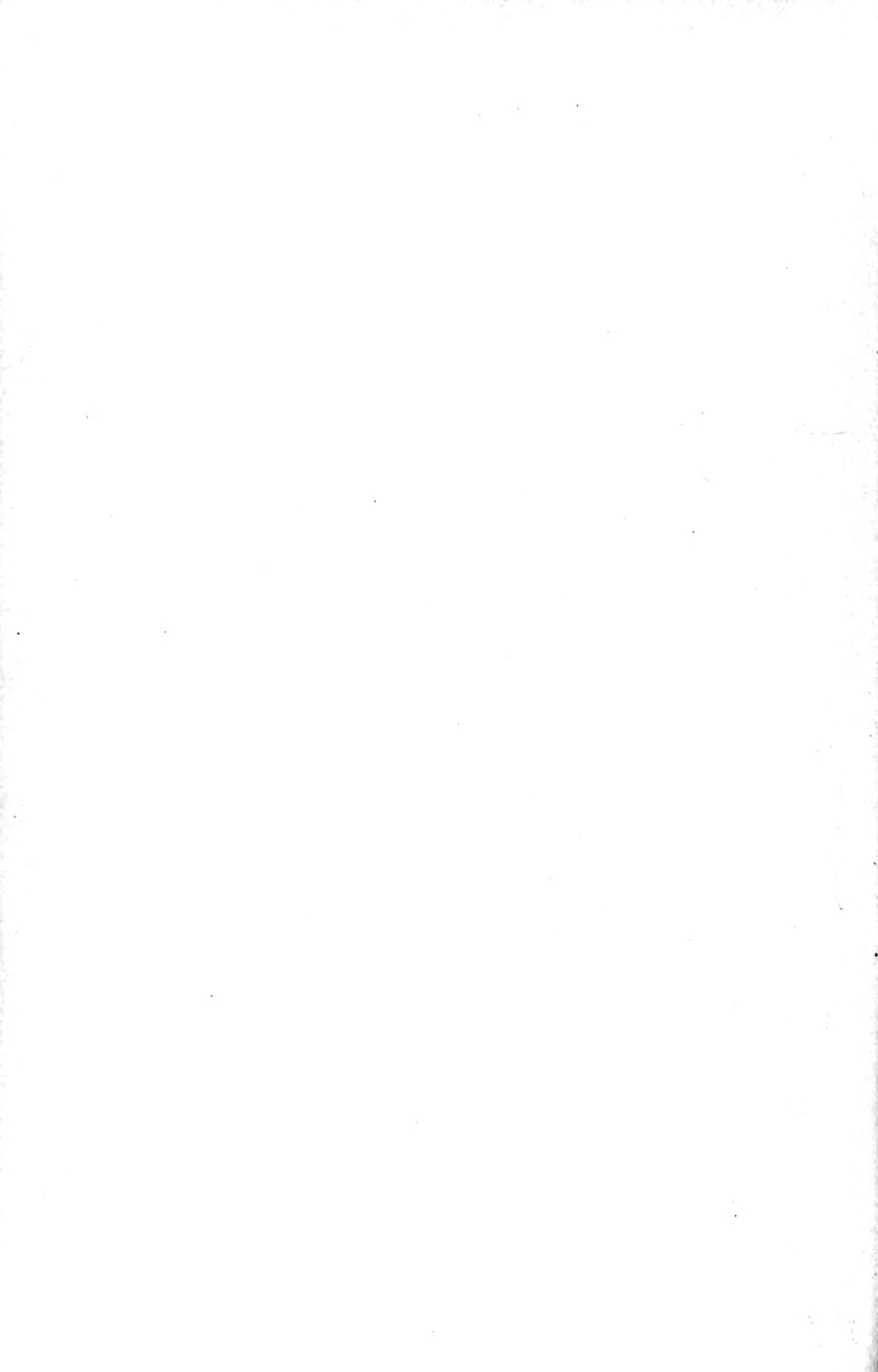
For example: I say that a tree expresses the potency or potentiality of the seed. This means three very concrete things. I expect the seed to have a future; I expect the future to be a tree—that is, a thing whose descriptive series is continuous with that already descriptive of the seed—and,

finally, I now look upon the seed as embodying the whole tree series now artificially present in my thought.

Of course, on the view of this paper the question of the ultimate origin of the universe may still come up for answer. Can there be an ultimate stopping-place anywhere in the career of the thing-world as a whole? Does not our position make it necessary that at any such stopping-place there should be some kind of filling drawn from yet antecedent history to give our statement of the conditions of origin any distinguishing character? It seems to me so. To say the contrary would be to do in favor of the prospective categories what we have been denying the right of the naturalist to do in favor of those of retrospect. Neither can proceed without the other. The only way to treat the problem of ultimate origin is not to ask it, as an isolated problem. Lotze says that the problem of philosophy is to require what reality is, not how it is made; and this will do if we remember that we must exhaust the empirical 'how' to get a notion of the empirical 'what,' and that there still remains over the 'prospect' which the same author has hit off in his famous saying, 'Reality is richer than thought.' To desiderate a what which has no how—this seems as contradictory as to ask for a how in terms of what is not. It is really this last chase of the 'how' that Lotze deprecates—and rightly.

Addenda. (1) *Further applications*: to the discussion of *freedom*; to the discussion of *ideals*; criticism of the general concept of *law* from this point of view; applications in *ethics* (*cf.* with Royce's distinction vs. 'world of description' and 'world of appreciation'); question of the notion of time (*i. e.*, is the distinction between the 'prospective' and 'retrospective' merely one of time, or does the notion of time find its genesis in this difference of mental attitude?)

(2) *References*: Ritchie, *Darwin and Hegel*, Chap. I; Royce, *Spirit of Modern Philosophy*, *in loc.* and *Int. Journ. of Ethics*, July 1895; Baldwin, *Mental Development: Methods and Processes*, Chaps. VII, XI, and *Int. Journ. of Ethics*, Oct. 1895.



SOMETHING MORE ABOUT THE 'PROSPECTIVE REFERENCE' OF MIND.

BY W. M. URBAN.

Fellow Princeton College.

In the present number of these CONTRIBUTIONS (preceding paper), Prof. Baldwin handles the problem of the completeness and satisfactoriness of the purely scientific answer as to the nature of the functions of knowledge. After showing the impossibility inherent in the very nature of the scientific historical categories of their saying the last word about any organized developing real, he applies the argument, *a fortiori*, to those developing reals which we call the functions of consciousness. Any thing of organization is only known by its activities, and my present conception of it is of the sum of its known activities up to the present moment. This is the scientific or historical view of a thing, or to use Prof. Baldwin's term, the 'retrospective reference' of mind. Under this view we can determine the 'how,' the manner of the development of a given thing; but does this give us the right to consider its past history the whole reality, the 'what' of the object of our study? Assuredly not, for we are immediately confronted with a new series of activities, which could not be predicted and which may change our entire conception of the thing. Thus was reached, by an elaboration of this idea, a theory which makes an element of teleology necessary to the worth of the historical fragments themselves. It is seen that the mind works equally under the category of description or retrospective reference, and teleology or prospective reference, if it wishes to conceive the 'what,' the reality, of a thing. One must remain a positivist, concern himself alone with the 'how' and give up the problem of the 'what,' if he denies the validity of the prospective way of looking at things; at least so should he do, if he would be consistent. And this is especially true of the functions of mind, which to know aright implies not only an understanding of their historical evolution, or of their present epistemological meaning, but likewise of the ideal end toward which they point.

But says the Naturalist: All this is true enough psychologically; yet this very prospective way of looking at things, on account of the possession of which you are dissatisfied with the historical categories, can be shown to have been naturally evolved, and, proud as it is, must owe its existence to the very past which it claims to transcend.

"If the mind has developed under constant stimulus from the exter-

nal world, and if its progress consists essentially in a more and more adequate representation in consciousness of relations already existing in the external world, then it follows that these internal representations can never do more than reflect the historical events of experience." How then can there be *any* phase of reality not subject to plain statement in terms of natural law? This is, however, but a new attempt to state the whole nature of a still active developing real in terms of its past, in this case the category of teleology itself. But the error rises likewise from a second and more subtle cause, namely, the failure to recognize the real relation between the historical categories and teleology, as it is deeply rooted in the psychology of knowledge.

This relation we may state, at least tentatively, in the following way: What we call the category of teleology is simply an induction from or a statement in historical terms of, just those elements in each of the historical categories that escape our description. Or, better, it is an *attempt* so to describe these prospective indescribable elements. This may seem to be so many words, or, if to be understood, to be a direct violation of our principle which says that the prospective reference must not be put into historical terms. But let us explain. In our study of the 'what' of mind, its 'behavior generalized,' we find one peculiarity about its activity, that is not open to observation as in the case of other organized developing things. It is true that every growing, moving organism will have much more to tell us of its nature years hence; but while, as we have seen, this may then throw our past reckoning out of count, at present it tells us nothing of the future. It is nothing more than a 'vague pressure toward the infinite.' But in the activities of mind we think there is something more. They have, as it were, taken us into confidence and revealed to us their hopes for the perfect, the highest, the absolute. Each historical category, as expressed in the judgments of time, space, causality, etc., contains, we shall attempt to show, a 'strain of prospective reference,' which is the very life-blood of its function. Spencer recognizes this infinite reference of the categories, but fails to make use of its implications for his theory of knowledge, seeing in it only an argument for his metaphysical assumption of an unknowable but absolute ground. We may very properly ask why do these categories look toward an absolute, of which we know nothing; why, if they have nothing but the phenomenal in themselves, do they look for that with which they have no kinship? Extend these modes of thought to infinity, and unless there be something of the absolute in their constitution, the journey will have failed to bring them there. As a matter of fact this infinite prospective reference has not only a meaning for meta

physics, but for the very psychology of knowledge itself; it is the moving principle of the categories, the constitutive element in their activity.

To discover this we must analyze a little more minutely the psychological character of the 'infinite prospective reference.' The Old Psychology¹ placed among the fundamental intuitions of mind, as fulfilling in inductive search the criteria of universality and necessity, the two categories of teleology and the infinite. In a general way, this seems to be true to the facts of psychology; but their close relation to each other and to the other categories of mind is not indicated. From a psychological point of view the intuitions of the infinite and of teleology are really one and the same, or rather have their roots in the same psychological principle. The intuition of the infinite possible future is simply the prospective reference in its 'first intention' devoid of reflection or application to the explanation of particular phenomena. The idea of telos or end is understood, however, when the vague, infinite reference of mind is reflected upon in connection with the application of the retrospective categories to the explanation of the particular phenomena of the world series.

This makes clearer the conception already brought forward that the teleological principle in mind is simply the prospective reference of all the historical categories, brought under one descriptive term. For when we apply any one of the descriptive categories like time or causation to particular phenomena, this vague infinite reference compels us to look forward as well as backward, and as we are then dealing with particular phenomena or representations, the end or telos of this infinite reference must likewise be of the nature of a representation, if it is to explain the representations, and thus is the element of ideality or teleology introduced. Against the objection, already suggested, to thus characterizing the general prospective reference, or teleology, as the prospective reference of all the historical categories, put under one general term, the answer can be made that such a description is only symbolic; for we are simply describing it negatively, as that part of the retrospective categories that forever escapes description in their own terms, in terms of natural law.

It now remains for us to make good, by psychological analysis of the retrospective categories, the claim that each contains this strain of 'prospective reference.' For then we shall have shown that teleology is a constitutive element in each, and, in the second place, secured a new point of view from which to consider the problem of knowledge.

That we may not take our categories at random—and also for another reason which will appear later—in prosecuting this research, let

¹ James McCosh, for instance.

us make use of the schematism of Schopenhauer's 'Vierfache Wurzel.' Following the static analysis of Kant, he proceeds to analyze the laws of *Vorstellen*—that narrow knife-edge of representations that lies between the two halves of the universe, subject and object—into four distinct classes, each of which has its own category and is ruled by a particular application of the 'Law of Ground.' Beginning, then, with the most mechanical of the categories, those of the second class, Space and Time (and for the reason that they are so mechanical, we shall find them the least propitious for our search), let us see if they do not contain also a strain of prospective reference.

The space of our study, it must be remembered, is not the space of geometry, of the so-called pure intuition, from whatever source that may come, but of the empirical intuition involved in our intuition of the external world as it may be shown to be historically evolved—in short, psychological space. For it is only this space which is a category of description, of history. Here, it is true, as well as in the sphere of geometry, the law of ground is simply the law of place, which says that any point determines as ground the position of every other point. But when this law of ground is applied geometrically it is essentially a retrospective, reflective, point of view, and is discernible only by reflectively impressing upon the empirical vision the laws of an abstract geometrical space. It is a necessity, which, just like logical necessity, is of the second intention; the law of the simple space intuition, as of all intuition of reality, is simply, as Paulsen has shown, an aesthetic 'Zusammenhang' or harmony.¹

Now what is the nature of this primary empirical space intuition, which we hold in common with lower forms of the animal world? Its chief characteristic is that it is subjective and psychological. It is an intuition, an outreaching from a particular 'here.' It becomes such by the very fact that it has the 'here.' Space without the 'here' is objective and geometrical. As empirical intuition it may be studied from two points of view, that of natural history, and, secondly, that of its meaning for the intuiting consciousness. As historically evolved, as seen under the aspect of natural causation, there is no reason for doubting that the empirical space consciousness is, as Spencer claims, but a more complex expression of the primitive adjustments of rudimentary organisms to environment. The only thing to be avoided is the tendency to become metaphysical, to leave the outer world of adjustments and find a metaphysical explanation

¹ 'Einleitung in die Philosophie,' p. 229. "Man kann es nicht stark genug betonen: Notwendigkeit ist im logiken Denken, aber nicht in der Natur; alle Naturgemässigkeit ist spontane Zusammenhang aller Teile."

for space in time or still lower in sensation. But the historical side is not the whole of the category. This gives its past. It has also, as we have seen, its epistemological present with its sharply defined 'law of ground' for dealing reflectively with the details of the intuition. It has also, finally, a future reference, a teleological meaning for the 'here,' from which the spatialization goes out. If genetically, we must construe the space intuition as a growing complex of adjustments to environment, we surely cannot say, *a priori*, that its development is complete. As a matter of fact, the synthesis is constantly growing and including new elements in its grasp. To be sure, the geometrical law of ground always does remain ruling, as a matter of history. But the reason we can say that things are necessarily in certain relations of place is simply because our historical experience of space has been such as to make this law of ground always applicable. But space as a function is nothing more than a growing grasp of the manifold of experience, and its only principle from the point of view of its prospective reference is a certain æsthetic harmony of place.

This teleological harmony—the ruling motive of the activity of space intuition—has come about on the following wise. Or, rather, one should not say come about, but made its appearance to consciousness. In the primitive animal the motive to the rudimentary adjustment to environment was an external one, the pressing of sensational environment upon him and thus the necessity of getting into harmony with it. In the spiritual human consciousness, however, the motive to spatialization with the extension of the category is the harmonization of all representations of a spatial nature, no matter by what means they have entered consciousness, in one all-inclusive ken. To this end it works not alone through sight and touch, which are the historical media of the intuition, but by the imaginative use of the mathematical symbols. Can it be said that the planets are not in my space because I have not measured their distances with the naked eye, and can only express their relations in the borrowed symbols of numbers? If so, then the house across the river, which I see from my window, is not in my space. For it is quite sure that geometrically its relation to the river is quite different from that which it holds in my perspective.

It becomes, then, mere foolishness to attempt to take all of the teleology out of the dynamic space intuition, to separate it from its empirical content, and subject it as a dead, statical *res completa*, to analysis; for contradictions immediately develop themselves, such as all keen critical thinkers from Zeno to Bradley have had no difficulty in

bringing against its reality. The reality of space exists, however, for the *intuiting subject*, before whom lies the spacial ideal, unconscious, perhaps, of finding in the *composition* of all the representations that have entered his spacial consciousness, a *place* for each in harmony with the great whole. Its ideal, its striving, is ever to overcome the limitations of the individual 'here,' and bring all reality that is external, the limitless world of a limitless space, into the ken of the knowing subject.

In the category of time the prospective reference is still more clearly shown. Here again we must distinguish between the time of mathematics and that of the empirical intuition with its 'now;' for only as it is related to this empirical 'now' is time the form of inner experience. This gives it the psychological character of an intuition, just as did the 'here' in the case of space. Succession is its law, to be sure, but as pure succession, independent of the 'now' of the intuition, it offers to reflection the same sort of difficulties as did space. Its nature refuses to be completely stated in retrospective terms. Bradley's criticism shows here likewise that, taken as a *res completa*, abstracted from its content and from the dynamic synthesis which is its nature, succession immediately develops intellectual contradictions. If the 'now' is but a point in the succession, and through its mediation one attempts to understand the connection of the past with the future, the 'now' will itself break up into atomistic and mutually repelling moments, so that the series will fall into contradiction. The reality of time consists, however, in the fact that it goes out as a dynamic synthesis from a 'now;' and the latter, instead of being a point of connection between the past and future of a series, is in reality the measure of our grasp upon the changing content of consciousness. The reason that the present can be a bridge between the past and the future is simply that in a vague indefinite sense it already feels the future. Historically, time, like space, was evolved through the reaction of primitive sensibility upon a manifold of sensations and was simply the successful attempt to hold them in its grasp. But the motive of time is now no longer one of sensation; it has to do with the harmonious grouping of *all* the contents of consciousness, no matter by what means they have entered. Not all are equally definitely placed, for the law here is not one of simple succession, but rather an æsthetic principle of temporal subordination according as they have meaning for the 'now.' This now is continually prospective and is ever looking forward to the wider complex which it will grasp in the hand of the future 'now.' And as the category of time develops genetically, the

'specious present,' by its growing richness of meaning, marks what of the flowing stream the individual has been able to synthesize, and again points to an intuition of things which shall grasp all in a timeless 'now.'¹

We now seek to discover the prospective reference in the two important retrospective categories of science; namely, Causation and Identity, or (lest Identity have a too metaphysical sound) the 'Same and the Different,' according to Mr. Spencer's terminology. Causation, the typical category of the Understanding, is, as an intuition, dependent upon time and space relations; but, when considered intellectually, it is an attempt to account rationally for change in space and time. But if we take the temporal relations existing between A and B, and try analytically to discover a real bond between them, we find, as Bradley points out, the same difficulties that appeared in the case of space and time, in fact, in relations of any kind. The A and B will persist in falling apart, for every attempt to introduce a mediating term ends in further disremp-tion. But that this is so, follows from a static and analytical view of what is a dynamic intuition of the subject—from a strange oblivion to the prospective element in this, as in all intuitions. Lotze, equally well, saw the difficulties that gather around the causal relation when it is analyzed statically into its merely spatial and temporal conditions. For once analyzed, the space and time as well as the causal idea itself are then seen only under the retrospective point of view. For consider, that the judgment of causation is primarily not due to a definite knowledge of the space and time relations. These are only analyzed after the intuition has taken place, in order to give analytical *grounds* for the intuitive judgment. In order to explain the causal judgment itself and, indeed, in order to make it consistent and rational when analyzed into its grounds, the element of teleology or organization must be recognized in it. Says Lotze in his *Metaphysic*: "The natures of things that act on each other, the inner states in which, for the moment, they happen to be, and the exact relations which exist between them, all constitute the complete ground or reason from which the resulting effect issues. Thus the consequence is contained in the reason." This is, of course, only discoverable, however, analytically in retrospective thinking. But, he continues, there is resident in the notion of causation, "the idea of some one plan, which is the complex of reality, which only once completes itself and nowhere hovers as a universal law over an indefinite number of instances, and

¹ Prof. A. T. Ormond 'Basal Concepts in Philosophy.' Chapter on Time.

which assigns to each state of facts that consequence which belongs to it as a further step in the realization of the one history.¹” This is the essential prospective reference of the category. It is this persuasion that in the harmony of the whole there is a necessary place for every experience of nature in relation to the others, that compels us to order the particulars under this rubric of causal relations. As an intuition this category presents to us a union of the prospective elements of both time and space, so that it seeks a harmony which includes in its plan both relations of place and of succession. As a matter of fact we do tacitly assume such a state of affairs, for every time we make an hypothesis, under the guidance of which we seek to discover causal relations, we rest upon the teleological element in our causal notion, which says to us that the particular facts *must mean* something like this hypothesis.

In regard to the typical category of the Reason, Identity, or, in its empirical expression, the ‘same and the different,’ only a few words are necessary. Natural science has very properly followed Hume in saying that in the sphere of perception, ‘first intention,’ there is no such thing as identity, but only close resemblance; and he is likewise perfectly justified in saying that these empirical judgments, historically considered, may be all reduced to habit and custom. But that ideal identity, which lies at the root of our judgments, the ideal which is so strong that we are always compelled to say that particulars are the same, although our experience afterwards (when we historically and analytically investigate the grounds for the judgment) invariably shows us that we were mistaken and had to do only with close resemblances—this side of the judgment requires other explanation than that of history, of custom or habit. It is really none other than the prospective reference to be found in this category; absolute identity is the distant ideal to which in its empirical expression the judgment never attains. Like a will-o’-the-wisp, it always escapes us, and, when we come up to our actual judgments and historically examine them they are seen to be concerned alone with close resemblances. But the genetic development of this category in an individual consciousness shows a closer and closer approximation to the ‘norm’ or ideal, showing that it does function as a regulation element in experience.

To attempt to show this prospective element in the sphere of ethics or in the will would be gratuitous, for motive, end, is the peculiar law of activity in this sphere. All empirical expressions of the will can be

¹ Metaphysic, p. 107.

understood only under the law of motivation. Whatever be its historical origin, the *existence* of a prospective 'must' in this sphere is never denied; it is in the historical categories that the problem of the prospective reference lies, for here, so it is thought, 'is,' actuality, expresses all.

So much for the psychological analysis of the categories themselves, by means of which we were to discover in their very constitution a strain of prospective reference—not only an infinite reference which points vaguely to an absolute ground, but their very life-blood, the withdrawal of which causes them to fall into pieces, giving us only appearance and illusion.

This is not so very different from the Platonic doctrine that all knowledge is only a remembrance, long since held for philosophical poesy. That doctrine is, however, but a symbolic way of expressing a fact that cannot fail to impress the mind that ponders the problem of knowledge. Is the present, individual knowing consciousness simply a spider at the end of a thread of its own spinning; or is there an instinct which determines the point to which that thread shall reach, a vital living connection with the consciousness that lies in the future time as well as with that of the historic past? How else shall I express those prospective judgments that do not seem to implicate will but only memory? The past alone does not explain them, 'nervous habit' and 'social custom' express only one side of the truth. Paradoxical and vague as the terms may seem, the prospective element in our knowledge functions can best be described as a *future forward memory* which, equally with the past, governs the activity of the present.

This becomes still more clear if these categories be united under some more ultimate one. It is in the basal category of sufficient reason, which has its peculiar law in each of these retrospective categories, that the prospective reference is most clearly marked. On its historical side, as an evolved psychological principle, it is explainable in terms of 'nervous habit' and 'accommodation'; it is the simple psychological principle of *interest*, with reactions made definite by habit. As an epistemological principle it is also seen under historic categories, for the law of ground in these different spheres of space and time, causality or the understanding, identity as typical of the reason, and motivation in the case of the will, is only discoverable when these judgments have taken their place as states in the historical, psychological series. For the descriptive terms of universality and necessity by which we test them are only discoverable in an inductive study of the static consciousness as instanced in the case of both Kant and the Natural Realists. In the case both of history and analysis we

look upon them as definite formulas or laws and by that very fact are compelled to put them under retrospective categories. But the principle of sufficient reason, as well as the particular categories in which it finds application, has a third and more ultimate side. As such it is simply the dynamic impulse to knowledge which presses on to further and more complete synthesis of mental content, using the categories as its instruments; it is prospective always; its grounds only coming into conscious recognition when the judgments are viewed historically. But now arises a most important question. If historically Sufficient Reason is nothing more than nervous habit, if its epistemological grounds are likewise purely retrospective, what can be said of its prospective reference, except that it is a blind forward impulse? Of what value is it to have shown the individual categories to be prospective in their nature, if the active principle which gets them in motion cannot be defined more definitely than that it is an impulse to know? Have we not gotten back again to the 'vague infinite' reference, into which we attempted to infuse an element of teleology? The strength of this criticism cannot be well overrated, and at first it may seem that, in having escaped the relativity that arises out of the natural history view of Spencer, we have fallen into the pessimistic fatalism of Schopenhauer. For this is none other than the position of this famous Kantian. Epistemologically the categories are absolutely valid in their own spheres, for phenomena, but they are simply necessary unchangeable mirrors through which the otherwise blind Will looks upon itself. But all movement is in Will; therefore no teleology to knowledge, for Will is blind. What difference for knowledge whether the principle that has brought its categories into being is one of blind force, operating under the law of natural selection, or a blind irrational evil, with no meaning in its movements?

Now, it cannot be denied that from one point of view there is an element of blind fatalism in the psychological principle of Sufficient Reason. The *act* of judgment itself, which is the expression of the subjective impulse called Sufficient Reason, is really a leap into the dark, in its first movement.¹ Its synthesis of elements is always prospective, and it is only in the light of this synthesis, largely æsthetic, that the grounds arise upon which we develop our reasons for the same. But by this time the judgment has already become an event

¹ Kant has the same idea of the Judgment (*Kritik der reinen Vernunft*, Ed. 1781, p. 78): "Die Synthesis überhaupt ist die blosse Wirkung der Einbildungskraft, einer blinden, obgleich unentbehrlichen Function der Seele, ohne die wir überall gar keine Erkenntniss haben würden, der wir uns aber selten nur einmal bewusst sind."

of history. So that the synthetic act itself is always without conscious grounds, always remains mysterious and illusive, making its necessity something almost fatalistic.

This is, undoubtedly, a true picture of the simple psychological impulse to knowledge, objectively considered. There is, however, a subjective concomitant, a reflex, so to speak, in the case of every judgment, which is so uniform in its meaning that it cannot fail to suggest a teleology to the forward movement of the psychological impulse itself. I refer to the element of necessity or *belief* with which we are compelled to pronounce a positive or negative judgment on any complex of form and content. In the sphere of 'first intention,' of sensation and perception, this is pure psychological necessity, or, in Prof. Baldwin's terms, 'reality feeling.' In the sphere of reflective judgment it becomes grounded or logical necessity, and its corresponding descriptive expression is belief. Now, it is important for our purpose that we see that there really exists no essential distinction between the absoluteness of these two necessities. Whatever may be the difference in their knowledge-content, as functions they are one and the same. In one the grounds are in the elements of the percept; in the other they lie in the conceptual relations of the elements in the judgment; but in each case it is a necessary response to a complex of form and content, and the response itself, as long as it remains undisturbed by any new elements of content, is absolute. Sigwart recognizes this in his doctrine of the necessity of all judgments; although the grounds in one may be psychological, while in another logical. Likewise Newman, in his 'Grammar of Assent,' argues keenly for the essential likeness of both kinds of assent, although 'inferences' may afterward enhance the value of the belief for the logical understanding. This is belief in all its aspects, when viewed as a psychological function. But is not this also as fatal and irrational as Sufficient Reason as a psychological impulse? Yes, viewed alone as a function it is.

Yet *forces* in the psychological sphere are as dark and inexplicable as in the physical. It is only as a *bond* connecting the concept of the movements of the earth and its surrounding planets that Gravitation has any meaning. As a pure force it is absolutely without any content for thought—must be relegated to the limbo of fantastic powers of enchantment and wilful activity. In the same way the pure reflex function of belief has no meaning in our study of consciousness, unless it be a *bond* between two elements of content that are ideal. Thus to say that belief is the reflex movement of con-

sciousness upon any complex of form and content describes it psychologically; but it is only when we conceive it as a *bond between the knowing self and its complexes of content* that it has any but a descriptive meaning for us.

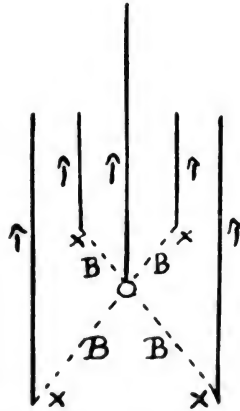
As a matter of fact, belief is essentially an act of appropriation to the *subject*, of that which Sufficient Reason, as an impulse to knowledge, has brought before the bar of consciousness. Belief is, above all, self-reference. This self-reference of belief is always manifest to one who is not prejudiced in favor of a sensational philosophy, and it is not without meaning that both Hume and Spencer find difficulty in giving even a satisfactory psychological explanation of belief. Now my final aim is simply this: to show that *the continual self-reference of belief is the bond which unites the movement of Sufficient Reason, otherwise irrational, to a developing self, whose ideal is the end toward which the impulse to knowledge, in Sufficient Reason, is blindly moving, and that this teleology is what gives meaning to the prospective reference of the categories, which this teleology has generated.* The psychological forces of Sufficient Reason and Belief are blind only as forces abstracted from the ideal self-content to which they relate.

But to make good this claim, this self-reference must be analytically shown to be psychologically true—from the lowest form of judgment to the highest. First in the reality-feeling that accompanies sensation and perception; here we must not fall into the error of the intellectual Idealist, who commits the ‘psychologist’s fallacy’ of making every feeling *explicitly* for a knowing self. Yet we must believe that the self-reference is at least implicit, else how (on the side of knowledge) could sensations ever be held together long enough for comparison and for the emergence of relations. The sensation is not first of all for a self, consciously, but it points vaguely to a self for whom it will become explicit later on in perception. As a matter of fact, recent studies in genetic psychology¹ point out that, in the development of the category of personality in the child, there are at first certain personality suggestions, very vague, to be sure, but nevertheless present, in the touch sensations that the infant receives in its earliest days. Already, in mere feeling, he learns to distinguish the personal in his external surroundings, and this reacts upon this budding notion of the self. As we pass from one higher synthesis to another, the self-reference becomes more marked. Time connects in a series the vanishing experiences, and by the mechanism of memory affords

¹Professor Baldwin’s ‘Mental Development in the Child and the Race.’

he possibility of an empirical self, which in turn by that constant increase of its grasp, points to a self which shall see all things *sub specie æternitatis*. Space brings with it the external world, both personal and impersonal; and by setting this over against the subject he further intensifies the self notion. With the advent of the category of causation comes a fuller notion of the self, for here energy is interpreted in terms of the activity of the self as revealed in the acts of will. The growth from the perception of close resemblances to the judgment of identity again brings the identical self into view as the norm and source of the judgment. The self in these last categories always reacts in the form of belief, and all these relations thus believed in are taken up and unified by the knowledge of a self as the source from which they depend and the end for which they have meaning. So much Kant saw, from a purely statical and analytical point of view. Whatever may be the metaphysical worth of the category of the self, it is at least the conceptual source of unity for all the other categories of consciousness.

There are thus discoverable two important lines of 'prospective reference': a main line in the development of the self-notion, and a number of independent forward references in the particular categories themselves—a constitutive element in their growth. Each of these lower categories is in turn connected by the self-reference of belief to the category of the self as the developing *motif* of the whole movement of Sufficient Reason. The following diagram will show this more clearly:



The x's are the four principal retrospective categories, each with its prospective reference. The o is the category of the self, with its

forward reference always in advance of the others. And by the bonds of Belief, the B's, each category is involved, in each of its activities or judgments, in the movement of the self, which is the richest category of consciousness and can then be used to interpret the others.

But suppose we seek for this unifying and explaining self as something among the complexes of content of which it is the ground and end. We shall then be looking for that which is the prospective reference of all the categories—all the syntheses of consciousness—among states of mind that have already taken their place in the historical empirical series. Our self, which seemed so much *en évidence* as it functioned ideally, has now withdrawn its support from the mechanical or retrospective categories, or, to employ a better figure, has fallen into lifeless dust among their atomic and disintegrated materials. But the self is just that point which can never be past, and for that reason can never be treated historically or found phenomenally. Just because it is the prospective reference of the empirical self, by which the latter is to be explained, does it refuse to be contained within those empirical limits. As Bradley argues, endeavoring to reduce the self like the other categories to illusion, psychologically (or under historic categories) the identity of the self or *ego* cannot be determined; for it must then be put somewhere within the temporal series and suffer the fate of time in his critical hands. But for that reason shall we call it illusion! If so we are reducing to illusion that which a study of the development of consciousness has shown to be the one element which has saved the whole movement from irrationality and confusion.

What then is the self? How is it to be construed in this connection? It is the empirical states, but more. As far as the retrospective categories can take hold of it is this the self. Nor is alone the unmediated intuition of the will the essential self, as Schopenhauer claims,¹ for this cannot explain the teleological function of the self in consciousness. Such an intuition has no ideal element whatever. It is pure present or past and can only be stated in terms of experienced acts of that will. This intuition, however, is a part of my self-consciousness, in that it gives me my notion of self as active and forceful. There is yet a third element of prospective or ideal significance which, just because it is prospective, will almost escape all statement in descriptive terms. It is that æsthetic harmony of our conscious states which we project as an ideal, that confidence (which is such a *ground-motif* of self-conscious life) that every element of

¹Vierfache Wurzel, end of paragraph 42.

consciousness has a meaning for the general harmony. This may, perhaps, be better suggested by a figure. The detail of the landscape before me is made up of rocks, trees, etc. As such, when I come up to them and subject them to study under the categories of description, they lose all the meaning that lay in the grouping of the perspective. The very value of the perspective is the æsthetic unity in which it reduces mere detail to its place in the whole. The same way of looking at things may be applied to consciousness. There is an element in the self whose very value lies in the æsthetic reduction of the indefiniteness which pervades the detail. This real psychological characteristic of consciousness can never be stated in descriptive terms, for as soon as we approach the scene with the instruments of science we have nothing but gross, crass details bound by nothing but mechanical laws. The prospective side of the self always escapes description, although for that reason it is no less an important psychological characteristic. Though rejoicing in the freedom from the thralldom of a metaphysical conception of the self as substance, he who fails to see nothing more in self-consciousness than an aggregate of empirical states, or an unmediated intuition of a feeling called will, is not yet entirely free from the chains of the 'stuff' idea.

With the preceding discussion before our eyes, there seems now some hope, if not of correcting the faults, at least of understanding the weakness of some current epistemological ideas. At the present day the static analysis of Kant has been extended in three important directions, each of these movements being animated by the important modern conceptions of the flux of things. To view knowledge as an activity, and, above all, as a development, to infuse into the rigidity of the intellectual categories the life and movement which appears in the volitional sphere—this has been the *motif* since the Kantian disreption of the Pure from the Practical Reason. Historically the idealistic movement came first with a thought-evolution, in which the categories are the steps of a development, with the 'Idea' as its goal. The Evolution Theory in the hands of science, which knows no permanent or static forms in any of its spheres, makes of the absolute functions of Kant evolved products of the interaction of a primary sensibility with its environment. Like the *fauna* and *flora* of the biological world, they have taken their place historically according to natural law, and therefore get their whole meaning from the nature out of which they spring. Any forward movement is vague, and infinite in its possibilities. The Schopenhauerian conception which puts the whole movement of knowledge in the hands of a blind Will,

and conceives the categories as complete existences, is but a purely metaphysical restoration of the breach between the Reason and the Will.

This is, however, the fault with all these theories: the movement is externally and metaphysically explained. It is not grounded in a psychological analysis of the knowledge factors themselves and of the self in its relation to knowledge. In the case of the Idealists the actual empirical development of knowledge is reduced to a mere *conceptual* relation of ideas, and, to the extent that the psychological roots of the concept are not known, is unpsychological. The Natural Science view, in so far as it finds the *origin* of the knowledge processes in the interaction of subject and object, of sensibility and environment, and by this seeks to explain them, is also metaphysical, either naïvely dualistic or somewhat materialistic in its monism. The third union of Will and Knowledge by Schopenhauer, is, of course, purely metaphysical.

If then the ontological teleology of Hegel is untenable, there is left either the pure relativity of Spencer, or the absolutely blind unteleological movement of the Schopenhauerian Will.

There is every motive then to look for a teleological, prospective reference of mind, as a constitutive element in the retrospective categories themselves which the Kantian critique had looked upon as static and unchangeable: above all to give it a psychological basis, for the metaphysical application will not be far in the rear.

Though the preceding study may not have been in any way of the nature of a supply to this demand, it yet affords grounds, we are convinced, for a somewhat more emphatic repetition of the poetical but keenly intuitive protest of Emerson against the Kantian description of Intellect and Will: "Our intellections are mainly *prospective*. The immortality of man is as legitimately preached from the intellect as from the moral volitions. Every intellection is mainly prospective; its present value is its least."¹

¹Essay on 'Intellect.'

CONSCIOUSNESS AND EVOLUTION.¹

BY J. MARK BALDWIN.

The addresses to which we have already listened, by Professors James and Cope, have raised so many interesting questions, and the various aspects of the general problem have been so clearly formulated, that I shall confine myself to a few remarks upon the positions which these speakers have taken.

Professor Cope's position on the place of consciousness in evolution seems in the main the true one, as far as the question of fact is concerned. I agree with him that no adequate theory of the development of organic nature can be formulated without taking conscious states into account. The fact of adaptation requires on the part of the individual organism something equivalent to what we call consciousness in ourselves. But I do not think that the need of recognizing consciousness in connection with organic functions leads at all necessarily to the view that consciousness is a *causa vera* whose modes of action do not have physiological parallel processes in the brain and nerves. The alternatives are not really two only, automatism—a theory of mechanical causation of all movement, with the inference that consciousness is a by-product of no importance, and this *vera causa* view which makes consciousness a new force injected into the activities of the brain. There is another way of looking at the question, to which I return below.

With Professor Cope's view that the recognition of consciousness as a factor in evolution requires a Neo-Lamarckian theory of heredity I am not at all in accord. I have recently discussed the question apropos of Professor Cope's views in *Science* (Aug. 23, 1895). Instead of finding with Professor Cope that the emphasis of conscious function in evolution makes it necessary to recognize the Lamarckian factor, I think the facts point just the other way. As soon as there is much development of mind, the gregarious or social life begins; and in it we have a new way of transmitting the acquisitions of one generation to another, which tends to supersede the action—if it exists—of natural heredity in such transmission. This transmission by 'So-

¹Discussion (revised) before the Amer. Psychol. Assoc., at Philadelphia, Dec. 28, 1895.

cial Heredity' (as we may call the individual's gains by learning from society by imitation, instruction, etc.) is so universal a fact with vertebrates that we may, it seems to me, say at once that the arguments for Neo-Lamarckism drawn by Mr. Spencer and others from the phenomena of human progress, at least, are completely neutralized by it. And there are facts which show that the same state of things descends below man.

It is very probable, as far as the early life of the child may be taken as indicating the factors of evolution, that the main function of consciousness is to enable him to learn things which natural heredity fails to transmit; and with the child the fact that consciousness is the essential means of all his learning is correlated with the other fact that the child is the very creature for which natural heredity gives few independent functions. It is in this field only that I venture to speak with assurance; but the recognition of this influence has been reached by Weismann, Morgan and others on the purely biological side.

The instinctive equipment of the lower animals is replaced by the plasticity necessary for learning by consciousness. So it seems to me that the evidence points to some inverse ratio between the importance of consciousness as factor in development and the need of the inheritance of acquired characters as factor in development. This presumptive argument may be supplemented, I think, with positive refutations of the considerations which Professor Cope, Romanes, and others present for the view that the transmission of functions secured by consciousness requires the Lamarckian factor.¹

The examination of the biological evidence just cited by Mr. Cope in support of Neo-Lamarckism I am not competent to make; but there is present another distinguished biologist, Prof. Minot, from whom I hope we may hear.

There is one omission in Professor James' excellent division of our topic into its members—an omission whose importance may justify my bringing up a phase of the general question to which I think too much importance can hardly be attached. It is, in biological phrase, the *ontogenetic* question, the examination of development of consciousness in the individual, with a view to the generalization of results and their application to race-development. Professor Cope's emphasis on consciousness rests here, and it is well placed. In the life history of the organism we have the problem of development

¹See articles on *Heredity and Instinct*, *Science*, March 20 and April 10, '96; Prof. Cope's reply and my further note may be found in the *Amer. Naturalist*, April and May, '96. The detailed bearings of this 'factor' in evolution are set forth in an article in the *Amer. Naturalist*, June and July, 1896.

actually in a measure solved before us. The biologist recognizes this in his emphasis on embryology and also to a degree in his paleontology. But the psychologist has not realized the weapon he has both for biological and for psychological use in the mental development of the child. Moreover the biologist no less than the psychologist must needs resort to this field of investigation if he would finally settle the function of consciousness in evolution. The fossils tell nothing of any such factor as consciousness. Nor does the embryo. So, as difficult as the ontogenetic question is, it is one of the really hopeful fields on both sides. I may be allowed, therefore, to give a brief summary of certain results reached by this method in my own work; especially since it will set out more fully, even in its defects and inadequacies, the general bearing of this problem.

That there is some general principle running through all the conscious adaptations of movement which the individual creature makes is indicated by the very unity of the organism itself. The principle of Habit must be recognized in some general way which will allow the organism to do new things without utterly undoing what it has already acquired. This means that old habits must be substantially preserved *in the new functions*; that all new functions must be reached by gradual modifications. And we will all go further and say, I think, that the only way that these modifications can be got at all is through some sort of interaction of the organism with its environment. Now, as soon as we ask how the stimulations of the environment can produce new adaptive movements, we have the answer of Spencer and Bain—an answer directly confirmed, I think, without question, by the study both of the child and of the adult—by the selection of fit movements from excessively produced movements, *i. e.*, from *movement variations*. So granting this, we now have the further question: How do these movement variations come to be produced *when and where they are needed?*¹ And with it, the question: How does the organism *keep those movements going* which are thus selected, and *suppress* those which are not selected?

Now these two questions are the ones which the biologists fail to answer. And the force of the facts leads to the hypotheses of 'conscious force' of Cope, 'self-development' of Henslow, and 'directive

¹ This is just the question that Weismann seeks to answer (in respect to the supply of variations in forms which the paleontologists require), with his doctrine of 'Germinal Selection' (*Monist*, Jan., 1896). Why are not such applications of the principle of natural selection to variations *in the parts and functions of the single organism* just as reasonable and legitimate as is the application of it to variations in separate organisms?

tendency' or 'determinate variation' of the American school—all aspects of the new vitalism which just these questions and the facts which they rest upon are now forcing to the front. Have we anything definite, drawn from the study of the individual on the psychological side, to substitute for these confessedly vague biological phrases? Spencer gave an answer in a general way long ago to the second of these questions, by saying that in consciousness the function of pleasure and pain is just to keep some actions or movements going and to suppress others. The evidence of this seems to me to be coextensive, actually, with the range of conscious experience, however we may be disposed to define the physiological processes which are involved in pleasure and pain. Actions which secure pleasurable conditions to the organism are determined by the pleasure to be repeated, and so to secure the continuance of the pleasurable conditions; and actions which get the organism into pain are by the very fact of pain suppressed.

But as soon as we enquire more closely into the actual working of pleasure and pain reactions, we find an answer suggested to the first question also, *i. e.*, the question as to how the organism comes to make the kind and sort of movements which the environment calls for—the movement-variations when and where they are required. The pleasure or pain produced by a stimulus—and by a movement also, for the utility of movement is always that it secures stimulation of this sort or that—does not lead to diffused, neutral, and characterless movements, as Spencer and Bain suppose: this is disputed no less by the infant's movements than by the actions of unicellular creatures. There are characteristic differences in vital movements wherever we find them. Even if Mr. Spencer's undifferentiated protoplasmic movements had existed, natural selection would very soon have put an end to it. There is a characteristic antithesis between movements always. Healthy, overflowing, favorable, outreaching, expansive, vital effects are associated with pleasure; and the contrary, the withdrawing, depressive, contractive, decreasing, vital effects are associated with pain. This is exactly the state of things which a theory of the selection of movements from overproduced movements requires, *i. e.*, that increased vitality, represented by pleasure, should give excess movements, from which new adaptations are selected; and that decreased vitality represented by pain should to the reverse—draw off energy and suppress movement.

If, therefore, we say that here is a type of reaction which all vitality shows, we may give it a general descriptive name, *i. e.*, the

'Circular Reaction,' in that its significance for evolution is that it is not a random response in movement to all stimulations alike, but that it distinguishes in its very form and amount between stimulations which are vitally good and those which are vitally bad, tending to retain the good stimulations and to draw away from and so suppress the bad. The term 'circular' is used to emphasize the way such a reaction tends to keep itself going, over and over, by reproducing the conditions of its own stimulation. It represents habit, since it tends to keep up old movements; but it secures new adaptations, since it provides for the overproduction of movement-variations for the operation of selection. This kind of selection, since it requires the direct coöperation of the organism itself, I have called 'Organic Selection.' It might be called 'motor' or even 'psychic' selection, since the part of consciousness, in the form of pleasure and pain, and later on experience generally, intelligence, etc., is so prominent.¹

This is a psychological attempt to discover the method of the individual's adaptations; it has detailed applications in the field of higher mental process, where imitation, volition, etc., give direct exemplifications of the circular type of reaction. But if the truth of it be allowed by the biologist for the individual's development, it follows from the doctrine of recapitulation that this type function shall run through all life. This would mean that something analogous to consciousness (as pleasure and pain, etc.) is coextensive with life, that the vital process itself shows a fundamental difference in movements—analogueous to the difference between pleasure-incited and pain-incited movements—and natural selection has reference to variations in it. The biologist may say that this is too special—this difference of reaction—to be fundamental; so it may be. But then so is life special, very special!

Whatever we may say to such particular conclusions, they illustrate one of the topics which should be discussed by anyone, biologist or psychologist, who wants to find all the factors in evolution. There are some factors revealed in ontogenesis which do not appear in the current theories of phylogenetic evolution. Indeed, so far beside the mark are the biologists who are discussing heredity to-day that they

¹ See Chap. VII. on 'The Theory of Development' in my *Mental Development in the Child and the Race* (2d ed., 1895). I have prepared a new chapter (XVII.) for the German and French editions of this work, incorporating the positions which this view of ontogenetic development leads to in respect to heredity, as suggested in the article referred to in *Science*. (See the next paper, p. 155, below.) It seems to secure determinate variations in phylogeny, without the inheritance of acquired characters.

generally omit—except when they hit at each other—the two factors which the psychologist has to recognize; Social Heredity, for the transmission of socially-acquired characters, and Organic Selection, for the accommodations of the individual organism, and through them of ‘determinate variations’ in phylogeny.

Indeed, I do not see how either theory of heredity can get along without this appeal to ontogenesis. For if we agree in denying the inheritance of acquired characters, thus throwing the emphasis on variations, still it is only by the interpretation of ontogenetic processes and characters that any general theory of variations can be reached. Either experience causes the variations, as one theory of heredity holds; or it exemplifies them, as the other theory holds; in either case, it is the only sphere of fact to which appeal can be made if we would understand them. So why do biologists speculate so long and so loud on the question of the mode of transmission, when the question of the mode of acquisition is so generally neglected by them?

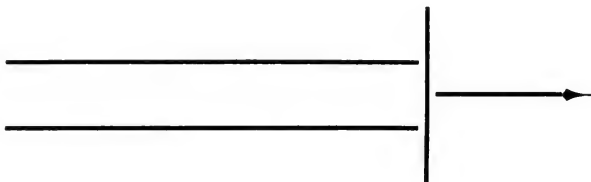
The only additional point which I may claim a little time to speak of is that to which Professor James referred in describing the current doctrines of the relation of mind and body. He described the view that consciousness does not in any way interfere with the activities of the brain, as the ‘automaton theory,’ and spoke as if in his mind a real automatism—the view which considers the brain processes as the sufficient statement of the grounds of all voluntary movement—was the outcome of any denial of causal energy in consciousness; in other words that there is no alternative to what is called the epi-phenomenon theory of consciousness except a theory holding that the law of conservation of physical energy is violated in voluntary movement.

Now this reduction of the possible views to two is, in my view, unnecessary and, indeed, impossible. In speaking of the antecedents of a voluntary movement we have to consider the entire group of phenomenal events which are always there when voluntary movement takes place; and among the phenomena really there the conscious state called volition is really there. To say that the same movement could take place without this state of consciousness is to say that a lesser group of phenomenal antecedents occurs in some cases and a larger group in other cases of the same event. Why not go to the other extreme, and say that the brain is not necessary to voluntary movement, since volition could bring about the movement without using the nervous processes to do it with? In his posthumous book on *Matter and Monism*, the late Mr. Romanes brings out this inadequacy of the automaton view, using the figure of an electro-magnet,

which attracts iron filings only when it is magnetized by the current of electricity. Whatever the electricity be, the magnet is a magnet only when it attracts iron filings; to say that it might do as much without the electricity would be to deny that it is a magnet; and the proof is found in the fact simply that it does not attract iron-filings when the current is not there. So the brain is not a brain when consciousness is not there; it could not produce voluntary movement, simply because, as a matter of fact, it does not. So consciousness does not, on the other hand, produce movement without a brain. The whole difficulty seems to lie, I think, in an illegitimate use of the word 'causation.' Professor Ladd seems to me to be correct in holding that such a conception as physical causation can not be applied beyond the sphere of things in which it has become the explaining principle, *i. e.*, in the objective, external world of things. The moment we ask questions concerning a group of phenomena which include more than these things, that moment we are liable to some new statement of the law of change in the group as a whole. Such a statement is the *third alternative* in this case; and it is the problem of the metaphysics of experience to find the category, or the most general principles of experience as a whole, both objective and subjective. This I do not care to discuss, but I am far from thinking that the automaton or epi-phenomenon man can argue his case with much force in this higher court of appeal.

The other extreme is represented by those writers who think that the revision of the law of causation can be made in the sphere of objective phenomenal action represented by the brain; and so claim that there is a violation of the principle of conservation of energy in a voluntary movement, an actual efficiency of some kind in consciousness itself for producing physical effects. This is as illegitimate as the other view—is it not? It seems to deny the results of all objective empirical science and so to sweep away the statements of law (on one side) on which the higher interpretation of the group of phenomena as a whole must be based. And it does it in favor of an equally empirical statement of law on the other side. I do not see how any result for the more complex system of events can be reached if we deny the only principles which we have in the partial groups. To do so is to attempt to interpret the objective in terms of the subjective factor in the entire group; and we reach by so doing a result which is just as partial as that which the epi-phenomenon man reaches in his mechanical explanation. Lotze made the same mistake long ago, but his hesitations on the subject showed that he appreciated the difficulty.

I agree with these writers in the claim that the mechanical view of causation can not be used as an adequate explaining principle of the whole personality of man; but for reasons of much the same kind it seems equally true that as long as we are talking of events of the external kind, *i. e.*, of brain processes, we can not deny what we know of these events as such.



The general state of the problem may be shown by the accompanying diagram, which will at any rate serve the modest purpose of indicating the alternatives. The line above, of the two parallels, may represent the statements on the psychological side which, on the theory of parallelism, mental science has a right to make; the lower of the parallels, the corresponding series of statements made by physics and natural science, includes the chemistry and physiology of the brain. Where they stop an upright line may be drawn to indicate the setting of the problem of interpretation in which both the other series of statements claim to be true; and the further line to the right then gives the phenomena and statements of them which we have to deal with when we come to consider man as a whole. Now my point is that we can neither deny either of the parallel lines in dealing with the phenomena of the single line to the right, nor can we take either of them as a sufficient statement of the farther problem which the line to the right proposes. To take the line representing the mechanical principles of nature and extend it alone beyond the upright is to throw out of nature the whole series of phenomena which belong in the upper parallel line and are not capable of statement in mechanical terms. And to extend the upper line alone beyond the upright is to allow that mechanical principles break down in their own sphere.

As to the interpretation of the single line to the right, it may always remain the problem that it now is. The best we can do is to get points of view regarding it; and the main progress of philosophy seems to me to be in getting an adequate sense of the conditions of the problem itself. From the more humble side of psychology, I think the growth of consciousness itself may teach us how the problem

comes to be set in the form of seemingly irreconcilable antinomies. The person grows both in body and mind, and this growth has to have two sides, the side facing toward the direction from which, the 'retrospective reference,' and the side facing the direction toward which, the 'prospective reference' of growth and the consciousness of growth. The positive sciences have by their very nature to face backwards, to look retrospectively, to be 'descriptive,' as the term is used by Professor Royce—these give the lower of our parallel lines. The moral sciences, so-called, on the other hand, deal with judgments, appreciations, organizations, expectations, and so represent the other, the 'prospective' mental attitude and its corresponding aspects of reality. This gives character largely to the upper one of our parallel lines. But to get a construction of the further line, the one to the right, is to ask for both these points of view at once—to stand at both ends of the line—at a point where description takes the place of prophecy and where reality has nothing further to add to thought. I believe for myself that the best evidence looking to the attainment of this double point of view is found just in the fact that we are able to compass both of these functions in a measure at once; and that in our own self-consciousness we have an inkling of what that ultimate point of view is like.¹ I do not mean to bring up points in philosophy; but it is to me the very essence of such a contention in philosophy that it is a comprehension of both aspects of phenomenal reality and not the violation or denial of either of them.

¹I may refer to the extended use made of this general antithesis in my paper in these CONTRIBUTIONS, Vol. I., No. 3, and to the philosophical considerations based on it by Mr. W. M. Urban in the same number.



A NEW FACTOR IN EVOLUTION

BY J. MARK BALDWIN.

In several recent publications I have developed, from different points of view, some considerations which tend to bring out a certain influence at work in organic evolution which I venture to call "a new factor." I give below a list of references¹ to these publications and shall refer to them by number as this paper proceeds. The object of the present paper is to gather into one sketch an outline of the view of the process of development which these different publications have hinged upon.

The problems involved in a theory of organic development may be gathered up under three great heads: Ontogeny, Phylogeny, Heredity. The general consideration, the "factor" which I propose to bring out, is operative in the first instance, in the field of *Ontogeny*; I shall consequently speak first of the problem of *Ontogeny*, then of that of *Phylogeny*, in so far as the topic dealt with makes it necessary, then of that of *Heredity*, under the same limitation, and finally, give some definitions and conclusions.

¹ References:

(1). *Imitation: a Chapter in the Natural History of Consciousness, Mind* (London), Jan., 1894. Citations from earlier papers will be found in this article and in the next reference.

(2). *Mental Development in the Child and the Race* (1st. ed., April, 1895; 2nd. ed., Oct., 1895; Macmillan & Co. The present paper expands an additional chapter (Chap. XVII) added in the German and French editions and to be incorporated in the third English edition.

(3). *Consciousness and Evolution, Science*, N. Y., August, 23, 1895; reprinted printed in the *AMERICAN NATURALIST*, April, 1896.

(4). *Heredity and Instinct* (I), *Science*, March 20, 1896. Discussion before N. Y. Acad. of Sci., Jan. 31, 1896.

(5). *Heredity and Instinct* (II), *Science*, April 10, 1896.

(6). *Physical and Social Heredity, Amer. Naturalist*, May, 1896.

(7). *Consciousness and Evolution, Psychol. Review*, May, 1896. Discussion before Amer. Psychol. Association, Dec. 28, 1895.

I.

Ontogeny: "*Organic Selection*" (see ref. 2, chap. vii).—The series of facts which investigation in this field has to deal with are those of the individual creature's development; and two sorts of facts may be distinguished from the point of view of the *functions which an organism performs in the course of his life history*. There is, in the first place, the development of his heredity impulse, the unfolding of his heredity in the forms and functions which characterize his kind, together with the congenital variations which characterize the particular individual—the phylogenetic variations, which are constitutional to him; and there is, in the second place, the series of functions, acts, etc., *which he learns to do himself in the course of his life*. All of these latter, the *special modifications which an organism undergoes during its ontogeny*, thrown together, have been called "acquired characters," and we may use that expression or adopt one recently suggested by Osborn,² "ontogenic variations" (except that I should prefer the form "ontogenetic variations"), if the word variations seems appropriate at all.

Assuming that there are such new or modified functions, in the first instance, and such "acquired characters," arising by the law of "use and disuse" from these new functions, our farther question is about them. And the question is this: How does an organism come to be modified during its life history?

In answer to this question we find that there are three different sorts of ontogenic agencies which should be distinguished—each of which works to produce ontogenetic modifications, adaptations, or variations. These are: first, the physical agencies and influences in the environment which work upon the organism to produce modifications of its form and functions. They include all chemical agents, strains, contacts, hindrances to growth, temperature changes, etc. As far

² Reported in *Science*, April 3rd.; also used by him before N. Y. Acad. of Sci., April 13th. There is some confusion between the two terminations "genic" and "genetic." I think the proper distinction is that which reserves the former, "genic," for application in cases in which the word to which it is affixed qualifies a term used *actively*, while the other, "genetic" conveys similarly a *passive* signification; thus agencies, causes, influences, etc., and "ontogenic phylogenic, etc.," while effects, consequences, etc., and "ontogenetic, phylogenetic, etc."

as these forces work changes in the organism, the changes may be considered largely "fortuitous" or accidental. Considering the forces which produce them I propose to call them "physico-genetic." Spencer's theory of ontogenetic development rests largely upon the occurrence of lucky movements brought out by such accidental influences. Second, there is a class of modifications which arise from the spontaneous activities of the organism itself in the carrying out of its normal congenital functions. These variations and adaptations are seen in a remarkable way in plants, in unicellular creatures, in very young children. There seems to be a readiness and capacity on the part of the organism to "rise to the occasion," as it were, and make gain out of the circumstances of its life. The facts have been put in evidence (for plants) by Henslow, Pfeffer, Sachs; (for micro-organisms) by Binet, Bunge; (in human pathology) by Bernheim, Janet; (in children) by Baldwin (ref. 2, chap. vi.) (See citations in ref. 2, chap. ix, and in Orr, *Theory of Development*, chap. iv.) These changes I propose to call "neuro-genetic," laying emphasis on what is called by Romanes, Morgan and others, the "selective property" of the nervous system, and of life generally. Third, there is the great series of adaptations secured by conscious agency, which we may throw together as "psycho-genetic." The processes involved here are all classed broadly under the term "intelligent," i. e., imitation, gregarious influences, maternal instruction, the lessons of pleasure and pain, and of experience generally, and reasoning from means to ends, etc.

We reach, therefore, the following scheme:

Ontogenetic Modifications.

Ontogenic Agencies.

- | | |
|-----------------------------|--------------------|
| 1. Physico-genetic. | 1. Mechanical. |
| 2. Neuro-genetic. | 2. Nervous. |
| 3. Psycho-genetic. | 3. Intelligent. |
| | Imitation. |
| | Pleasure and pain. |
| | Reasoning. |

Now it is evident that there are two very distinct questions which come up as soon as we admit modifications of function

and of structure in ontogenetic development: first, there is the question as to how these modifications can come to be adaptive in the life of the individual creature. Or in other words: What is the method of the individual's growth and adaptation as shown in the well known law of "use and disuse?" Looked at functionally, we see that the organism manages somehow to accommodate itself to conditions which are favorable, to repeat movements which are adaptive, and so to grow by the principle of use. This involves some sort of selection, from the actual ontogenetic variations, of certain ones—certain functions, etc. Certain other possible and actual functions and structures decay from disuse. Whatever the method of doing this may be, we may simply, at this point, claim the law of use and disuse, as applicable in ontogenetic development, and apply the phrase, "Organic Selection," to the organism's behavior in acquiring new modes or modifications of adaptive function with its influence of structure. The question of the method of "Organic Selection" is taken up below (IV); here, I may repeat, we simply assume what every one admits in some form, that such adaptations of function—"accommodations" the psychologist calls them, the processes of learning new movements, etc.—*do occur*. We then reach another question, second; what place these adaptations have in the general theory of development.

Effects of Organic Selection.—First, we may note the results of this principle in the creature's own private life.

1. *By securing adaptations, accommodations, in special circumstances the creature is kept alive* (ref. 2, 1st ed., pp. 172 ff.). This is true in all the three spheres of ontogenetic variation distinguished in the table above. The creatures which can stand the "storm and stress" of the physical influences of the environment, and of the changes which occur in the environment, by undergoing modifications of their congenital functions or of the structures which they get congenitally—these creatures will live; while those which cannot, will not. In the sphere of neurogenetic variations we find a superb series of adaptations by lower as well as higher organisms during the course of ontogenetic development (ref. 2, chap. ix). And in the highest

sphere, that of intelligence (including the phenomena of consciousness of all kinds, experience of pleasure and pain, imitation, etc.), we find individual accommodations on the tremendous scale which culminates in the skilful performances of human volition, invention, etc. The progress of the child in all the learning processes which lead him on to be a man, just illustrates this higher form of ontogenetic adaptation (ref. 2, chap. x-xiii).

All these instances are associated in the higher organisms, and all of them unite to *keep the creature alive*.

2. By this means *those congenital or phylogenetic variations are kept in existence, which lend themselves to intelligent, imitative, adaptive, and mechanical modification during the lifetime of the creatures which have them*. Other congenital variations are not thus kept in existence. So there arises a more or less widespread series of *determinate variations in each generation's ontogenesis* (ref. 3, 4, 5).³

The further applications of the principle lead us over into the field of our second question, i. e., phylogeny.

II.

Phylogeny: Physical Heredity.—The question of phylogenetic development considered apart, in so far as may be, from that of heredity, is the question as to what the factors really

³ "It is necessary to consider further how certain reactions of one single organism can be selected so as to adapt the organism better and give it a life history. Let us at the outset call this process "Organic Selection" in contrast with the Natural Selection of whole organisms. . . . If this (natural selection) worked alone, every change in the environment would weed out all life except those organisms, which by accidental variation reacted already in the way demanded by the changed conditions—in every case new organisms showing variations, not, in any case, new elements of life-history in the old organisms. In order to the latter we would have to conceive . . . some modification of the old reactions in an organism through the influence of new conditions. . . . We are, accordingly, left to the view that the new stimulations brought by changes in the environment themselves modify the reactions of an organism. . . . The facts show that individual organisms do acquire new adaptations in their lifetime, and that is our first problem. If in solving it we find a principle which may also serve as a principle of race-development, then we may possibly use it against the 'all sufficiency of natural selection' or in its support" (ref. 2, 1st. ed., pp. 175-6.)

are which show themselves in evolutionary progress from generation to generation. The most important series of facts recently brought to light are those which show what is called "determinate variation" from one generation to another. This has been insisted on by the paleontologists. Of the two current theories of heredity, only one, Neo-Lamarckism—by means of its principle of the inheritance of acquired characters—has been able to account for this fact of determinate phylogenetic change. Weismann admits the inadequacy of the principle of natural selection, as operative on rival organisms, to explain variations when they are wanted or, as he puts it, "the right variations in the right place" (*Monist*, Jan., '96).

I have argued, however, in detail that the assumption of determinate variations of function in ontogenesis, under the principle of neurogenetic and psychogenetic adaptation, does away with the need of appealing to the Lamarckian factor. In the case i. g., of instincts, "if we do not assume consciousness, then natural selection is inadequate; but if we do assume consciousness, then the inheritance of acquired characters is unnecessary" (ref. 5).

"The intelligence which is appealed to, to take the place of instinct and to give rise to it, uses just these partial variations which tend in the direction of the instinct; so the intelligence *supplements* such partial co-ordinations, makes them functional; and *so keeps the creature alive*. In the phrase of Prof. Lloyd Morgan, this prevents the 'incidence of natural selection.' So the supposition that intelligence is operative turns out to be just the supposition which makes use-inheritance unnecessary. Thus kept alive, the species has all the time necessary to perfect the variations required by a complete instinct. And when we bear in mind that the variation required is not on the muscular side to any great extent, but in the central brain connections, and is a slight variation for functional purposes at the best, the hypothesis of use-inheritance becomes not only unnecessary, but to my mind quite superfluous" (ref. 4, p. 439). And for adaptations generally, "the most plastic individuals will be preserved to do the

advantageous things for which their variations show them to be the most fit, and the next generation will show an emphasis of just this direction in its variations" (ref. 3, p. 221).

We get, therefore, from Organic Selection, certain results in the sphere of phylogeny:

1. *This principle secures by survival certain lines of determinate phylogenetic variation in the directions of the determinate ontogenetic adaptations of the earlier generation.* The variations which were utilized for ontogenetic adaptation in the earlier generation, being thus kept in existence, are utilized more widely in the subsequent generation (ref. 3, 4). "Congenital variations, on the one hand, are kept alive and made effective by their use for adaptations in the life of the individual; and, on the other hand, adaptations become congenital by further progress and refinement of variation in the same lines of function as those which their acquisition by the individual called into play. But there is no need in either case to assume the Lamarckian factor" (ref. 3). And in cases of conscious adaptation: "We reach a point of view which gives to organic evolution a sort of intelligent direction after all; for of all the variations tending in the direction of an adaptation, but inadequate to its complete performance, *only those will be supplemented and kept alive which the intelligence ratifies and uses.* The principle of 'selective value' applies to the others or to some of them. So natural selection kills off the others; and the *future development at each stage of a species' development must be in the directions thus ratified by intelligence.* So also with imitation. Only those imitative actions of a creature which are useful to him will survive in the species, for in so far as he imitates actions which are injurious he will aid natural selection in killing himself off. So intelligence, and the imitation which copies it, will set the direction of the development of the complex instincts even on the Neo-Darwinian theory; and in this sense we may say that consciousness is a 'factor'" (ref. 4).

2. *The mean of phylogenetic variation being thus made more determinate, further phylogenetic variations follow about this mean, and these variations are again utilized by Organic Selection for on-*

togenetic adaptation. So there is continual phylogenetic progress in the directions set by ontogenetic adaptation (ref. 3, 4, 5). "The intelligence supplements slight co-adaptations and so gives them selective value; but it does not keep them from getting farther selective value as instincts, reflexes, etc., by farther variation" (ref. 5). "The imitative function, by using muscular co-ordinations, supplements them, secures adaptations, keeps the creature alive, prevents the 'incidence of natural selection,' and so gives the species all the time necessary to get the variations required for the full instinctive performance of the function" (ref. 4). But, "Conscious imitation, while it prevents the incidence of natural selection, as has been seen, and so keeps alive the creatures which have no instincts for the performance of the actions required, nevertheless does not subserve the utilities which the special instincts do, nor prevent them from having the selective value of which Romanes speaks. Accordingly, on the more general definition of intelligence, which includes in it all conscious imitation, use of maternal instruction, and that sort of thing—no less than on the more special definition—we still find the principal of natural selection operative" (ref. 5).

3. *This completely disposes of the Lamarckian factor as far as two lines of evidence for it are concerned.* First, the evidence drawn from function, "use and disuse," is discredited; since by "organic selection," the reappearance, in subsequent generations, of the variations first secured in ontogenesis is accounted for without the inheritance of acquired characters. So also the evidence drawn from paleontology which cites progressive variations resting on functional use and disuse. Second, the evidence drawn from the facts of "determinate variations;" since by this principle we have the preservation of such variations in phylogeny without the inheritance of acquired characters.

4. *But this is not Preformism in the old sense; since the adaptations made in ontogenetic development which "set" the direction of evolution are novelties of function in whole or part (although they utilize congenital variations of structure). And it is only by the exercise of these novel functions that the*

creatures are kept alive to propagate and thus produce further variations of structure which may in time make the whole function, with its adequate structure, congenital. Romanes' argument from "partial co-adaptations" and "selective value," seem to hold in the case of reflex and instinctive functions (ref. 4, 5), as against the old preformist or Weismannist view, although the operation of Organic Selection, as now explained, renders them ineffective when urged in support of Lamarckism. "We may imagine creatures, whose hands were used for holding only with the thumb and fingers on the same side of the object held, to have first discovered, under stress of circumstances and with variations which permitted the further adaptation, how to make use of the thumb for grasping opposite to the fingers, as we now do. Then let us suppose that this proved of such utility that all the young that did not do it were killed off; the next generation following would be plastic, intelligent, or imitative, enough to do it also. They would use the same co-ordinations and prevent natural selection getting its operation on them; and so instinctive 'thumb-grasping' might be waited for indefinitely by the species and then be got as an instinct altogether apart from use-inheritance" (ref. 4). "I have cited 'thumb-grasping' because we can see in the child the anticipation, by intelligence and imitation, of the use of the thumb for the adaptation which the Simian probably gets entirely by instinct, and which I think an isolated and weak-minded child, say, would also come to do by instinct'" (ref. 4).

5. It seems to me also—though I hardly dare venture into a field belonging so strictly to the technical biologist—that *this principle might not only explain many cases of widespread "determinate variations" appearing suddenly, let us say, in fossil deposits, but the fact that variations seem often to be "discontinuous."* Suppose, for example, certain animals, varying, in respect to a certain quality, from a to n about a mean x . The mean x would be the case most likely to be preserved in fossil form (seeing that there are vastly more of them). Now suppose a sweeping change in the environment, in such a way that only the variations lying near the extreme n

can accommodate to it and live to reproduce. The next generation would then show variations about the mean n . And the chances of fossils from this generation, and the subsequent ones, would be of creatures approximating n . Here would be a great discontinuity in the chain and also a widespread prevalence of these variations in a set direction. This seems especially evident when we consider that the paleontologist does not deal with successive generations, but with widely remote periods, and the smallest lapse of time which he can take cognizance of is long enough to give the new mean of variation, n , a lot of generations in which to multiply and deposit its representative fossils. Of course, this would be only the action of natural selection upon "preformed" variations in those cases which did not involve positive changes, in structure and function, *acquired in ontogenesis*; but in so far as such ontogenetic adaptations were actually there, the extent of difference of the n mean from the x mean would be greater, and hence the resources of explanation, both of the sudden prevalence of the new type and of its discontinuity from the earlier, would be much increased. This additional resource, then, is due to the "Organic Selection" factor.

We seem to be able also to utilize all the evidence usually cited for the functional origin of specific characters and groupings of characters. So far as the Lamarkians have a strong case here, it remains as strong if Organic Selection be substituted for the "inheritance of acquired characters." This is especially true where intelligent and imitative adaptations are involved, as in the case of instinct. This "may give the reason, e. g., that instincts are so often coterminous with the limits of species. Similar structures find the similar uses for their intelligence, and they also find the same imitative actions to be to their advantage. So the interaction of these conscious factors with natural selection brings it about that the structural definition which represents species, and the functional definition which represents instinct, largely keep to the same lines" (ref. 5).

6. It seems proper, therefore, to call the influence of Organic Selection "a new factor;" for it gives a method of deriving

the determinate gains of phylogeny from the adaptations of ontogeny without holding to either of the two current theories. *The ontogenetic adaptations are really new, not performed ; and they are really reproduced in succeeding generations, although not physically inherited.*

III.

Social Heredity.—There follows also another resource in the matter of development. In all the higher reaches of development we find certain co-operative or "social" processes which directly supplement or add to the individual's private adaptations. In the lower forms it is called gregariousness, in man sociality, and in the lowest creatures (except plants) there are suggestions of a sort of imitative and responsive action between creatures of the same species and in the same habitat. In all these cases it is evident that other living creatures constitute part of the environment of each, and many neuro-genetic and psycho-genetic accommodations have reference to or involve these other creatures. It is here that the principle of imitation gets tremendous significance ; intelligence and volition, also, later on ; and in human affairs it becomes social co-operation. Now it is evident that when young creatures have these imitative, intelligent, or quasi-social tendencies to any extent, they are able to pick up *for themselves*, by imitation, instruction, experience generally, the functions which their parents and other creatures perform in their presence. This then is a form of ontogenetic adaptation ; it keeps these creatures alive, and so produces determinate variations in the way explained above. It is, therefore, a special, and from its wide range, an extremely important instance of the general principle of Organic Selection.

But it has a farther value. *It keeps alive a series of functions which either are not yet, or never do become, congenital at all.* It is a means of extra-organic transmission from generation to generation. It is really a form of heredity because (1) *it is a handing down of physical functions ;* while it is not physical heredity. It is entitled to be called heredity for the further reason (2) *that it directly influences physical heredity in the way men-*

tioned, i. e., it keeps alive variations, thus sets the direction of ontogenetic adaptation, thereby influences the direction of the available congenital variations of the next generation, and so determines phylogenetic development. I have accordingly called it "Social Heredity" (ref. 2, chap. xii; ref. 3).

In "Social Heredity," therefore, we have a more or less conservative, progressive, ontogenic atmosphere of which we may make certain remarks as follows:—

(1) *It secures adaptations of individuals all through the animal world.* "Instead of limiting this influence to human life, we have to extend it to all the gregarious animals, to all the creatures that have any ability to imitate, and finally to all animals who have consciousness sufficient to enable them to make adaptations of their own; for such creatures will have children that can do the same, and it is unnecessary to say that the children must inherit what their fathers did by intelligence, when they can do the same things by intelligence" (ref. 6).

(2) *It tends to set the direction of phylogenetic progress by Organic Selection, Sexual Selection, etc., i. e., it tends not only to give the young the adaptations which the adults already have, but also to produce adaptations which depend upon social coöperation; thus variations in the direction of sociality are selected and made determinate.* "When we remember that the permanence of a habit learned by one individual is largely conditioned by the learning of the same habits by others (notably of the opposite sex) in the same environment, we see that an enormous premium must have been put on variations of a social kind—those which brought different individuals into some kind of joint action or coöperation. Whenever this appeared, not only would habits be maintained, but new variations, having all the force of double hereditary tendency, might also be expected" (ref. 3). Why is it, for example, that a race of Mulattoes does not arise faster, and possess our Southern States? Is it not just the social repugnance to black-white marriages? Remove or reverse *this influence of education, imitation, etc.,* and the result on phylogeny would show in our faces, and even appear in our fossils when

they are dug up long hence by the paleontologist of the succeeding aeons!

(3) *In man it becomes the law of social evolution.* "Weismann and others have shown that the influence of animal intercourse, seen in maternal instruction, imitation, gregarious coöperation, etc., is very important. Wallace dwells upon the actual facts which illustrate the 'imitative factor,' as we may call it, in the personal development of young animals. I have recently argued that Spencer and others are in error in holding that social progress demands use-inheritance; since the socially-acquired actions of a species, notably man, are socially handed down, giving a sort of 'social heredity' which supplements natural heredity" (ref. 4). The social "sport," the genius, is very often the controlling factor in social evolution. He not only sets the direction of future progress, but he may actually lift society at a bound up to a new standard of attainment (ref. 6). "So strong does the case seem for the Social Heredity view in this matter of intellectual and moral progress that I may suggest an hypothesis which may not stand in court, but which I find interesting. May not the rise of social life be justified from the point of view of a second utility in addition to that of its utility in the struggle for existence as ordinarily understood, the second utility, *i. e.*, of giving to each generation the attainments of the past which natural inheritance is inadequate to transmit. When social life begins, we find the beginning of the artificial selection of the unfit; and this negative principle begins to work directly in the teeth of progress, as many writers on social themes have recently made clear. This being the case, some other resource is necessary besides natural inheritance. On my hypothesis it is found in the common or social standards of attainment which the individual is fitted to grow up to and to which he is compelled to submit. This secures progress in two ways: First, by making the individual learn what the race has learned, thus preventing social retrogression, in any case; and second, by putting a direct premium on variations which are socially available" (ref. 3).

4. The two ways of securing development in determinate directions—the purely extra-organic way of Social Heredity, and the way by which Organic Selection in general (both by social and by other ontogenetic adaptations) secures the fixing of phylogenetic variations, as described above—seem to run parallel. Their conjoint influence is seen most interestingly in the complex instincts (ref. 4, 5). We find in some instincts completely reflex or congenital functions which are accounted for by Organic Selection. In other instincts we find only partial coördinations ready given by heredity, and the creature actually depending upon some conscious resource (imitation, instruction, etc.) to bring the instinct into actual operation. But as we come up in the line of phylogenetic development, both processes may be present *for the same function*; the intelligence of the creature may lead him to do consciously what he also does instinctively. In these cases the additional utility gained by the double performance accounts for the duplication. It has arisen either (1) by the accumulation of congenital variations in creatures which already performed the action (by ontogenetic adaptation and handed it down socially), or (2) the reverse. In the animals, the social transmission seems to be mainly useful as enabling a species to get instincts slowly in determinate directions, by keeping off the operation of natural selection. Social Heredity is then the lesser factor; it serves Biological Heredity. But in man, the reverse. Social transmission is the important factor, and the congenital equipment of instincts is actually broken up in order to allow the plasticity which the human being's social learning requires him to have. So in all cases both factors are present, but in a sort of inverse ratio to each other. In the words of Preyer, "the more kinds of co-ordinated movement an animal brings into the world, the fewer is he able to learn afterwards." The child is the animal which inherits the smallest number of congenital co-ordinations, but he is the one that learns the greatest number (ref. 2, p. 297).

"It is very probable, as far as the early life of the child may be taken as indicating the factors of evolution, that the main function of consciousness is to enable him to learn things which

natural heredity fails to transmit; and with the child the fact that consciousness is the essential means of all his learning is correlated with the other fact that the child is the very creature for which natural heredity gives few independent functions. It is in this field only that I venture to speak with assurance; but the same point of view has been reached by Weismann and others on the purely biological side. The instinctive equipment of the lower animals is replaced by the plasticity for learning by consciousness. So it seems to me that the evidence points to some inverse ratio between the importance of consciousness as factor in development and the need of inheritance of acquired characters as factor in development" (ref. 7).

"Under this general conception we may bring the biological phenomena of infancy, with all their evolutionary significance: the great plasticity of the mammal infant as opposed to the highly developed instinctive equipment of other young; the maternal care, instruction and example during the period of dependence, and the very gradual attainment of the activities of self-maintenance in conditions in which social activities are absolutely essential. All this stock of the development theory is available to confirm this view" (Ref. 3).

But these two influences furnish a double resort against Neolamarckism. And I do not see anything in the way of considering the fact of Organic Selection, from which both these resources spring, as being a sufficient supplement to the principle of natural selection. The relation which it bears to natural selection, however, is a matter of further remark below (V).

"We may say, therefore, that there are two great kinds of influence, each in a sense hereditary; there is *natural heredity* by which variations are congenitally transmitted with original endowment, and there is '*social heredity*' by which functions socially acquired (*i. e.*, imitatively, covering all the conscious acquisitions made through intercourse with other animals) are also socially transmitted. The one is phylogenetic; the other ontogenetic. But these two lines of hereditary influence are not separate nor uninfluential on each other. Congenital varia-

tions, on the one hand, are kept alive and made effective by their conscious use for intelligent and imitative adaptations in the life of the individual; and, on the other hand, intelligent and imitative adaptations become congenital by further progress and refinement of variation in the same lines of function as those which their acquisition by the individual called into play. But there is no need in either case to assume the Lamarkian factor" (ref. 4).

"The only hindrance that I see to the child's learning everything that his life in society requires would be just the thing that the advocates of Lamarkism argue for—the inheritance of acquired characters. For such inheritance would tend so to bind up the child's nervous substance in fixed forms that he would have less or possibly no unstable substance left to learn anything with. So, in fact, it is with the animals in which instinct is largely developed; they have no power to learn anything new, just because their nervous systems are not in the mobile condition represented by high consciousness. They have instinct and little else" (ref. 3).

IV.

The Process of Organic Selection.—So far we have been dealing exclusively with facts. By recognizing certain facts we have reached a view which considers ontogenetic selection an important factor in development. Without prejudicing the statement of fact at all we may enquire into the actual working of the organism in making its organic selections or adaptations. The question is simply this: how does the organism secure, from the multitude of possible ontogenetic changes which it might and does undergo, those which are adaptive? As a matter of fact, all personal growth, all motor acquisitions made by the individual, show that it succeeds in doing this; the further question is, how? Before taking this up, I must repeat with emphasis that the position taken in the foregoing pages, which simply makes the fact of ontogenetic adaptation a factor in development, is not involved in the solution of the further question as to how the adaptations are secured. But from the answer to this latter question we may get further light of the interpretation of the facts themselves. So we come to ask how

Organic Selection actually operates in the case of a particular adaptation of a particular creature (ref. 1; ref. 2, chap. vii, xiii; ref. 6, and 7).

I hold that the organism has a way of doing this which is peculiarly its own. The point is elaborated at such great length in the book referred to (ref. 2) that I need not repeat details here. The summary in this journal (ref. 6) may have been seen by its readers. There is a fact of physiology which, taken together with the facts of psychology, serves to indicate the method of the adaptations or accommodations of the individual organism. The general fact is that the organism concentrates its energies upon the locality stimulated, for the continuation of the conditions, movements, stimulations which are vitally beneficial, and for the cessation of the conditions, movements, stimulations, which are vitally depressing and harmful. In the case of beneficial conditions we find a general *increase of movement, an excess discharge of the energies of movement in the channels already open and habitual; and with this, on the psychological side, pleasurable consciousness and attention.* Attention to a member is accompanied by increased vasomotor activity, with higher muscular power, and a *general dynamogenic heightening in that member.* "The thought of a movement tends to discharge motor energy into the channels as near as may be to those necessary for that movement" (ref. 3). By this organic concentration and excess of movement many combinations and variations are rendered possible, from which the advantageous and adaptive movements may be selected for their utility. These then give renewed pleasure, excite pleasurable associations, and again stimulate the attention, and *by these influences the adaptive movements thus struck are selected and held as permanent acquisitions.* This form of concentration of energy upon stimulated localities, with the resulting renewal by movements of conditions that are pleasure-giving and beneficial, and the subsequent repetitions of the movements, is called the "circular reaction."⁴ (ref. 1, 2). It is the selective property which Romanes pointed

⁴ With the opposite (withdrawing, depressive affects) in injurious and painful conditions.

out as characterizing and differentiating life. It characterizes the responses of the organism, however low in the scale, to all stimulations—even those of a mechanical and chemical (physico-genic) nature. Pfeffer has shown such a determination of energy toward the parts stimulated even in plants. And in the higher animals it finds itself exactly reproduced in the nervous reaction seen in imitation and—through processes of association, substitution, etc.—in all the higher mental acts of intelligence and volition. These are developed phylogenetically as variations whose direction is constantly determined, by this form of adaptation in ontogenesis. If this be true—and the biological facts seem fully to confirm it—this is the adaptive process in all life, and this process is that with which the development of mental life has been associated.

It follows, accordingly, that the three forms of ontogenetic adaptation distinguished above—physico-genetic, neuro-genetic, psycho-genetic—all involve the sort of response on the part of the organism seen in this circular reaction with excess discharge; and we reach one general law of ontogenetic adaptation and of Organic Selection. “The accommodation of an organism to a new stimulation is secured—not by the selection of this stimulation beforehand (nor of the necessary movements)—but by the reinstatement of it by a discharge of the energies of the organism, concentrated as far as may be for the excessive stimulation of the organs (muscles, etc.) most nearly fitted by former habit to get this stimulation again (in which the “stimulation” stands for the condition favorable to adaptation). After several trials the child (for example) gets the adaptation aimed at more and more perfectly, and the accompanying excessive and useless movements fall away. This is the kind of selection that intelligence does in its acquisition of new movements” (ref. 2, p. 179; ref. 6).

Accordingly, *all ontogenetic adaptations are neurogenetic*.⁵ The general law of “motor excess” is one of *overproduction*; from movements thus overproduced, adaptations survive; these adaptations set the determinate direction of ontogenesis; and

⁵ Barring, of course, those violent compelling physical influences under the action of which the organism is quite helpless.

by their survival the same determination of direction is set in phylogenesis also.

The following quotation from an earlier paper (ref. 7) will show some of the bearings of this position :

"That there is some general principle running through all the adaptations of movement which the individual creature makes is indicated by the very unity of the organism itself. The principle of Habit must be recognized in some general way which will allow the organism to do new things without utterly undoing what it has already acquired. This means that old habits must be substantially preserved *in the new functions* ; that all new functions must be reached by gradual modifications. And we will all go further and say, I think, that the only way that these modifications can be got at all is through some sort of interaction of the organism with its environment. Now, as soon as we ask how the stimulations of the environment can produce new adaptive movements, we have the answer of Spencer and Bain—an answer directly confirmed, I think, without question, by the study both of the child and of the adult—i. e., by the selection of fit movements from excessively produced movements, that is, from *movement variations*. So granting this, we now have the further question : How do these movement variations come to be produced *when and where they are needed* ?⁶ And with it, the question : How does the organism *keep those movements going* which are thus selected, and *suppress* those which are not selected ?

"Now these two questions are the ones which the biologists fail to answer. But the force of the facts leads to the hypotheses of "conscious force," "self-development" of Henslow

⁶ This is just the question that Weismann seeks to answer (in respect to the supply of variations in forms which the paleontologists require), with his doctrine of "Germinal Selection" (*Monist*, Jan., 1896). Why are not such applications of the principle of natural selection to variations *in the parts and functions of the single organism* just as reasonable and legitimate as it is to variations in separate organisms ? As against "germinal selection," however, I may say, that in the cases in which ontogenetic adaptation sets the direction of survival of phylogenetic variations (as held in this paper) the hypothesis of germinal selection is in so far unnecessary. This view finds the operation of selection *on functions in ontogeny* the means of securing "variations when and where they are wanted;" while Weismann supposes competing germinal units.

and "directive tendency" of the American school—all aspects of the new Vitalism which just these questions and the facts which they rest upon are now forcing to the front. Have we anything definite, drawn from the study of the individual on the psychological side, to substitute for these confessedly vague biological phrases? Spencer gave an answer in a general way long ago to the *second* of these questions, by saying that in consciousness the function of pleasure and pain is just to keep some actions or movements going and to suppress others.

"But as soon as we enquire more closely into the actual working of pleasure and pain reactions, we find an answer suggested to the *first* question also, *i. e.*, the question as to how the organism comes to make the kind and sort of movements which the environment calls for—the *movement variations when and where they are required*. The pleasure or pain produced by a stimulus—and by a movement also, for the utility of movement is always that it secures stimulation of this sort or that—does not lead to diffused, neutral, and characterless movements, as Spencer and Bain suppose; this is disputed no less by the infant's movements than by the actions of unicellular creatures. There are characteristic differences in vital movements wherever we find them. Even if Mr. Spencer's undifferentiated protoplasmic movements had existed, natural selection would very soon have put an end to it. There is a characteristic antithesis in vital movements always. Healthy, overflowing, outreaching, expansive, vital effects are associated with pleasure; and the contrary, the withdrawing, depressive, contractive, decreasing, vital effects are associated with pain. This is exactly the state of things which the theory of selection of movements from overproduced movements requires, *i. e.*, that increased vitality, represented by pleasure, should give the excess movements, from which new adaptations are selected; and that decreased vitality represented by pain should do the reverse, *i. e.*, draw off energy and suppress movement.⁷

⁷ It is probable that the origin of this antithesis is to be found in the waxing and waning of the nutritive processes. "We find that if by an organism we

"If, therefore, we say that here is a type of reaction which all vitality shows, we may give it a general descriptive name, *i. e.*, the "Circular Reaction," in that its significance for evolution is that it is not a random response in movement to all stimulations alike, but that it distinguishes in its very form and amount between stimulations which are vitally good and those which are vitally bad, tending to retain the good stimulations and to draw away from and so suppress the bad. The term 'circular' is used to emphasize the way such a reaction tends to keep itself going, over and over, by reproducing the conditions of its own stimulation. It represents habit, since it tends to keep up old movements; but it secures new adaptations, since it provides for the overproduction of movement variations for the operation of selection. This kind of selection, since it requires the direct coöperation of the organism itself, I have called 'Organic Selection.'"

The advantages of this view seem to be somewhat as follows:

1. It gives a method of the individual's adaptations of function which is *one in principle with the law of overproduction and survival now so well established in the case of competing organisms.*

2. It reduces nervous and mental evolution to strictly parallel terms. The intelligent use of phylogenetic variations for functional purposes in the way indicated, puts a premium on variations which can be so used, and thus sets phylogenetic progress *in directions of constantly improved mental endowment.* The circular reaction which is the method of intelligent adapta-

mean a thing merely of contractility or irritability, whose round of movements is kept up by some kind of nutritive process supplied by the environment—absorption, chemical action of atmospheric oxygen, etc.—and whose existence is threatened by dangers of contact and what not, the first thing to do is to secure a regular supply to the nutritive processes, and to avoid these contacts. But the organism can do nothing but move, as a whole or in some of its parts. So then if one of such creatures is to be fitter than another to survive, it must be the creature which by its movements secures more nutritive processes and avoids more dangerous contacts. But movements toward the source of stimulation keep hold on the stimulation, and movements away from contacts break the contacts, that is all. Nature selects these organisms; how could she do otherwise? . . . We only have to suppose, then, that the nutritive growth processes are by natural selection drained off in organic expansions, to get the division in movements which represents this earliest bifurcate adaptation" (Ref. 2, p. 201).

tions is liable to variation in a series of complex ways which represent phylogenetically the development of the mental functions known as memory, imagination, conception, thought, etc. We thus reach a phylogeny of mind which proceeds in the direction set by the ontogeny of mind,⁸ just as on the organic side the phylogeny of the organism gets its determinate direction from the organism's ontogenetic adaptations. And since it is the one principle of Organic Selection working by *the same functions* to set the direction of both phylogenies, the physical and the mental, the two developments are not two, but one. Evolution is, therefore, not more biological than psychological (ref. 2, chap. x, xi, and especially pp. 383-388).

3. It secures the relation of structure to function required by the principle of "use and disuse" in ontogeny.

4. The only alternative theory of the adaptations of the individual are those of "pure chance," on the one hand, and a "creative act" of consciousness, or the other hand. Pure chance is refuted by all the facts which show that the organism does not wait for chance, but goes right out and effects new adaptations to its environment. Furthermore, ontogenetic adaptations are determinate; they proceed in definite progressive lines. A short study of the child will disabuse any man, I think, of the "pure chance" theory. But the other theory which holds that consciousness makes adaptations and changes structures directly by its *fiat*, is contradicted by the psychology of voluntary movement (ref. 4, 6, 7). Consciousness can bring about no movement without having first an adequate experience of that movement to serve on occasion as a stimulus to the innervation of the appropriate motor centers. "This point is no longer subject to dispute; for pathological cases show that unless some adequate idea of a former movement made by the same muscles, or by association some other idea which stands for it, can be brought up in mind the intelligence is helpless. Not only can it not make new movements; it can not even repeat old habitual movements. So we may say that intelligent adaptation does not create coördinations; it only

⁸ Prof. C. S. Minot suggests to me that the terms "ontopsychic" and "phylopsychic" might be convenient to mark this distinction.

makes functional use of coördinations which were alternatively present already in the creature's equipment. Interpreting this in terms of congenital variations, we may say that the variations which the intelligence uses are alternative possibilities of muscular movement" (ref. 4). So the only possible way that a really new movement can be made is *by making the movements already possible so excessively and with so many varieties of combination, etc., that new adaptations may occur.*

5. The problem seems to me to duplicate the conditions which led Darwin to the principle of natural selection. The alternatives before Darwin were "pure chance" or "special creation." The law of "overproduction with survival of the fittest" came as the solution. So in this case. Let us take an example. Every child has to learn how to write. If he depended upon chance movements of his hands he would never learn how to write. But on the other hand, he can not write simply by willing to do so; he might will forever without effecting a "special creation" of muscular movement. What he actually does is to *use his hand in a great many possible ways as near as he can to the way required*; and from these excessively produced movements, and after excessively varied and numerous trials, he gradually selects and fixes the slight successes made in the direction of correct writing. It is a long and most laborious accumulation of slight Organic Selections from over-produced movements (ref. for handwriting in detail, 2, chap. v; also 2, pp. 373, ff.).

6. The only resort left to the theory that consciousness is some sort of an *actus purus* is to hold that it *directs* brain energies or selects between possible alternatives of movement; but besides the objection that it is as hard to direct movement as it is to make it (for nothing short of a force could release or direct brain energies), we find nothing of the kind necessary. The attention is what determines the particular movement in developed organisms, and the attention is no longer considered an *actus purus* with no brain process accompanying it. The attention is a function of memories, movements, organic experiences. We do not attend to a thing because we have already selected it, or because the attention selects it; but *we*

select it because we—consciousness and organism—are attending to it. “It is clear that this doctrine of selection as applied to muscular movement does away with all necessity for holding that consciousness even directs brain energy. The need of such direction seems to me to be as artificial as Darwin showed the need of special creation to be for the teleological adaptations of the different species. This need done away, in this case of supposed directive agency as in that, the question of the relation of consciousness to the brain becomes a metaphysical one, just as that of teleology in nature became a metaphysical one; and it is not to much profit that science meddles with it. And biological as well as psychological science should be glad that it is so, should it not?” (ref. 6; and on the metaphysical question, ref. 7).

V.

A word on the relation of this principle of Organic Selection to Natural Selection. Natural Selection is too often treated as a positive agency. It is not a positive agency; it is entirely negative. It is simply a statement of what occurs when an organism does not have the qualifications necessary to enable it to survive in given conditions of life; it does not in any way define positively the qualifications which do enable other organisms to survive. Assuming the principle of Natural Selection in any case, and saying that, according to it, if an organism do not have the necessary qualifications it will be killed off, it still remains in that instance to find what the qualifications are which this organism is to have if it is to be kept alive. So we may say that *the means of survival is always an additional question* to the negative statement of the operation of natural selection.

This latter question, of course, the theory of variations aims to answer. The positive qualifications which the organism has arise as congenital variations of a kind which enable the organism to cope with the conditions of life. This is the positive side of Darwinism, as the principle of Natural Selection is the negative side.

Now it is in relation to the theory of variations, and not in relation to that of natural selection, that Organic Selection has

its main force. Organic Selection presents *a new qualification of a positive kind* which enables the organism to meet its environment and cope with it, while natural selection remains exactly what it was, the negative law that if the organism does not succeed in living, then it dies, and as such a qualification on the part of the organism, Organic Selection presents several interesting features.

1. If we hold, as has been argued above, that the method of Organic Selection is always the same (that is, that it has a natural method), being always accomplished by a certain typical sort of nervous process (*i. e.*, being always neuro-genetic), then we may ask whether that form of nervous process—and the consciousness which goes with it—may not be a variation appearing early in the phylogenetic series. I have argued elsewhere (ref. 2, pp. 200 ff. and 208 ff.) that this is the most probable view. Organisms that did not have some form of selective response to what was beneficial, as opposed to what was damaging in the environment, could not have developed very far; and as soon as such a variation did appear it would have immediate preëminence. So we have to say either that selective nervous property, with consciousness, is a variation, or that it is a fundamental endowment of life and part of its final mystery. "The intelligence holds a remarkable place. It is itself, as we have seen, a congenital variation; but it is also the great agent of the individual's personal adaptation both to the physical and to the social environment" (ref. 4).

"The former (instinct) represents a tendency to brain variation in the direction of fixed connections between certain sense-centers and certain groups of coördinated muscles. This tendency is embodied in the white matter and the lower brain centers. The other (intelligence) represents a tendency to variation in the direction of alternative possibilities of connection of the brain centers with the same or similar coördinated muscular groups. This tendency is embodied in the cortex of the hemispheres" (ref. 4).

2. But however that may be, whether ontogenetic adaptation by selective reaction and consciousness be considered a variation or a final aspect of life, it is a *life-qualification of a very*

extraordinary kind. It opens a new sphere for the application of the negative principle of natural selection upon organisms, *i. e.*, with reference to *what they can do*, rather than to what they are; to the new use they make of their congenital functions, rather than to the mere possession of the functions (ref. 2, pp. 202 f.). A premium is set on congenital plasticity and adaptability of function rather than on congenital fixity of function; and this adaptability reaches its highest in the intelligence.

3. It opens another field also for the operation of natural selection—still viewed as a negative principle—through the survival of particular overproduced and modified reactions of the organism, by which the determination of the organism's own growth and life-history is secured. If the young chick imitated the old duck instead of the old hen, it would perish; it can only learn those new things which its present equipment will permit—not swimming. So the chick's own possible actions and adaptations in ontogeny have to be selected. We have seen how it may be done by a certain competition of functions with survival of the fit. But this is an application of natural selection. I do not see how Henslow, for example, can get the so-called "self-adaptations"—apart from "special creation"—which justify an attack on natural selection. Even plants must grow in determinate or "select" directions in order to live.

4. So we may say, finally, that Organic Selection, while itself probably a congenital variation (or original endowment) works to secure new qualifications for the creature's survival; and its very working proceeds by securing a new application of the principle of natural selection to the possible modifications which the organism is capable of undergoing. Romanes says: "it is impossible that heredity can have provided in advance for innovations upon or alterations in its own machinery during the lifetime of a particular individual." To this we are obliged to reply in summing up—as I have done before (ref. 2, p. 220)—we reach "just the state of things which Romanes declares impossible—heredity providing for the modification of its own machinery. Heredity not only leaves the future free for modifications, it also provides a method of

life in the operation of which modifications are bound to come."

VI.

The Matter of Terminology.—I anticipate criticism from the fact that several new terms have been used in this paper. Indeed one or two of these terms have already been criticised. I think, however, that novelty in terms is better than ambiguity in meanings. And in each case the new term is intended to mark off a real meaning which no current term seems to express. Taking these terms in turn and attempting to define them, as I have used them, it will be seen whether in each case the special term is justified; if not, I shall be only too glad to abandon it.

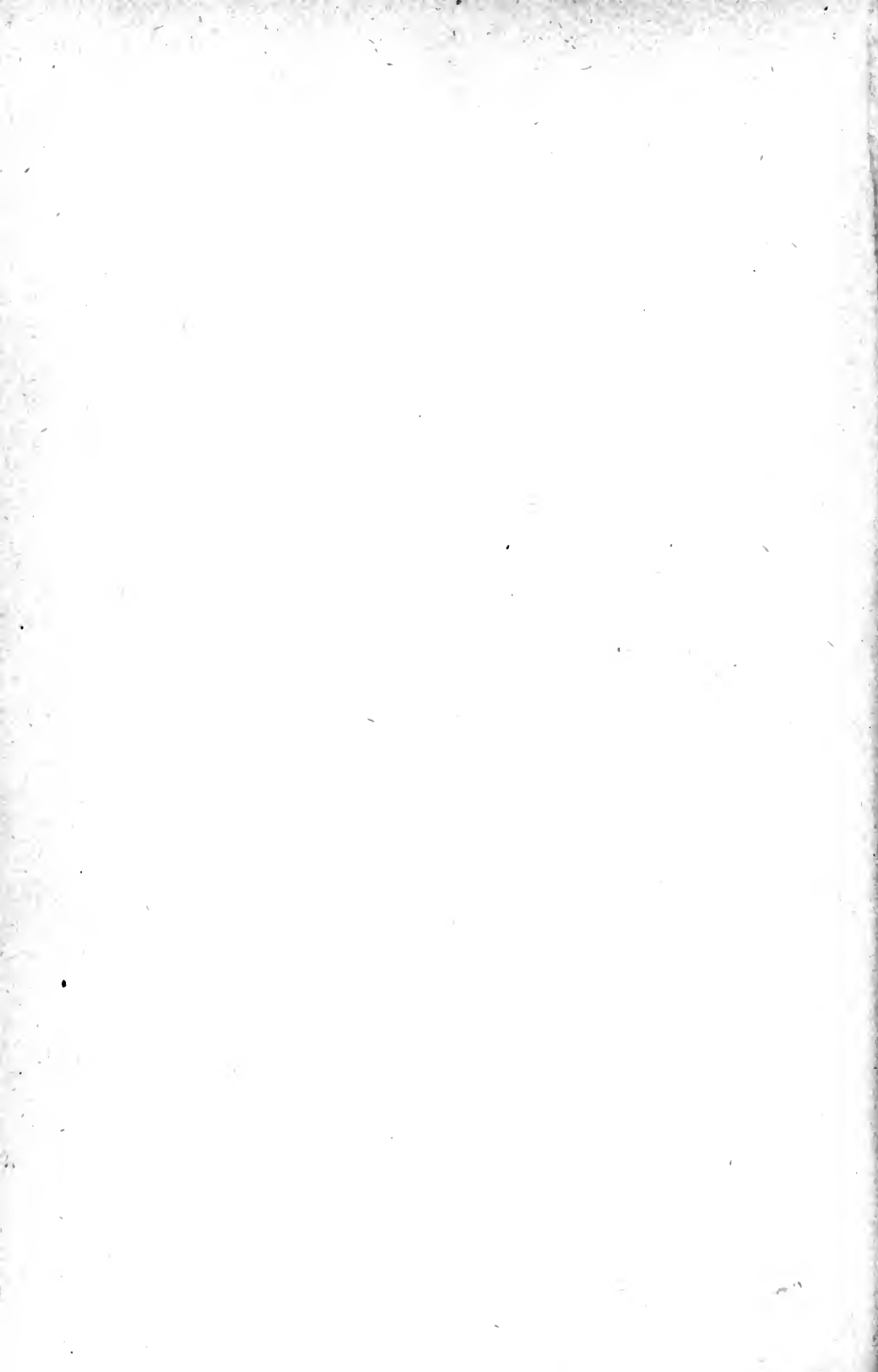
Organic Selection.—The process of ontogenetic adaptation considered as keeping single organisms alive and so securing determinate lines of variation in subsequent generations. Organic Selection is, therefore, a general principle of development which is a direct substitute for the Lamarckian factor in most, if not in all instances. If it is really a new factor, then it deserves a new name, however contracted its sphere of application may finally turn out to be. The use of the word "Organic" in the phrase was suggested from the fact that the organism itself coöperates in the formation of the adaptations which are effected, and also from the fact that, in the results, the organism is itself selected; since those organisms which do not secure the adaptations fall by the principle of natural selection. And the word "Selection" used in the phrase is appropriate for just the same two reasons.

Social Heredity.—The acquisition of functions from the social environment, also considered as a method of determining phylogenetic variations. It is a form of Organic Selection but it deserves a special name because of its special way of operation. It is really heredity, since it influences the direction of phylogenetic variation by keeping socially adaptive creatures alive while others which do not adapt themselves in this way are cut off. It is also heredity since it is a continuous influence from generation to generation. Animals may be kept alive let us say in a given environment by social co-

operation only; these transmit this social type of variation to posterity; *thus social adaptation sets the direction of physical phylogeny and physical heredity is determined in part by this factor.* Furthermore the process is all the while, from generation to generation, aided by the continuous chain of extra-organic or purely social transmissions. Here are adequate reasons for marking off this influence with a name.

The other terms I do not care so much about. "Physico-genetic," "neuro-genetic," "psycho-genetic," and their correlatives in "genic," seem to me to be convenient terms to mark distinctions which would involve long sentences without them, besides being self-explanatory. The phrase "circular reaction" has now been welcomed as appropriate by psychologists. "Accommodation" is also current among psychologists as meaning single functional adaptations, especially on the part of consciousness; the biological word "adaptation" refers more, perhaps, to racial or general functions. As between them, however, it does not much matter.⁹

⁹ I have already noted in print (ref. 4 and 6) that Prof. Lloyd Morgan and Prof. H. F. Osborn have reached conclusions similar to my main one on Organic Selection. I do not know whether they approve of this name for the "factor;" but as I suggested it in the first edition of my book (April, 1895) and used it earlier, I venture to hope that it may be approved by the biologists.



PRINCETON CONTRIBUTIONS

TO

PSYCHOLOGY.

EDITED BY

J. MARK BALDWIN,
Stuart Professor of Psychology.

VOL II. 1897-8.

PRINCETON, N. J.
THE UNIVERSITY PRESS.
PRICE 50 CENTS.

CONTENTS OF VOL. II.

	PAGE.
I. <i>Ueber die Wahrnehmung zweier Punkte mittelst des Tastsinnes</i> : G. A. TAWNEY, - - - - -	I
II. <i>The Negative in Logic</i> : A. T. ORMOND, - - - - -	61
III. <i>The Psychology of Sufficient Reason</i> : W. M. URBAN, -	77
IV. <i>Determinate Evolution</i> : J. MARK BALDWIN, - - - -	90
V. <i>Studies from the Princeton Psychological Laboratory</i> : (VI)	
<i>The Reaction Time of Counting</i> : H. C. WARREN, -	99
(VII) <i>Experiments on the Successive Double-Point Threshold</i> : G. A. TAWNEY AND C. W. HODGE, -	121
VI. <i>President's Address on Selective Thinking</i> : J. MARK BALDWIN, - - - - -	145
VII. <i>Studies from the Princeton Laboratory</i> : (VIII) <i>Preliminary Report on the Temperature Sense</i> : J. F. CRAWFORD, - - - - -	169

Ueber
die Wahrnehmung zweier Punkte
mittelst des Tastsinnes,

mit Rücksicht auf die Frage der Uebung
und die Entstehung der Vexirfehler.

von

Guy A. Tawney,

Ex-Fellow.

Trotz der zahlreichen eingehenden Abhandlungen im Gebiete des Tastsinnes, die seit den Weber'schen Versuchen erschienen sind, bleibt die Psychologie bis heute noch über die Erklärung mancher Thatsachen dieses Gebietes unentschieden. Die vorliegende Arbeit setzte sich zunächst die Aufgabe, den Einfluss der Uebung auf das Erkennen zweier Punkte durch den Tastsinn zu untersuchen. Eine andere Frage stellte sich aber bald in den Weg: dies war der oft bemerkte und bis jetzt kaum erklärte Vexirfehler. Während der Untersuchung dieser zwei Erscheinungen wurden einige neue Thatsachen gefunden, deren Bedeutung einerseits für die allgemeine Theorie der Sinneswahrnehmung, anderseits für die Methode aller Tastsinnversuche von Wichtigkeit zu sein scheint. Im Anschluss an diese Untersuchungen sollen daher schließlich einige kritische Bemerkungen über die Methode gegeben werden.

Als Versuchspersonen, denen hiermit zugleich mein bester Dank ausgesprochen sei, nahmen an diesen Untersuchungen Theil die Herren: G. M. Stratton, W. P. Ladd, M. Arrer, Dr. F. Kiesow, M. Chamdanjian, H. Eber, Dr. P. Mentz, Dr. Brahn, E. Mosch, A. C. Perry, E. M. Weyer und S. J. Franz, außerhalb des Instituts Herr Rev. S. G. Hefelbower. Es sei noch bemerkt, dass die Untersuchungen ursprünglich gemeinsam mit Herrn Dr. C. H. Judd begonnen wurden.

Als Hilfsapparat wurde mit wenigen Ausnahmen ein einfacher

aus Messing angefertigter Zirkel gebraucht, dessen Schenkel mit Gelenken versehen waren, damit die Spitzen rechtwinklig zur Oberfläche der Hautstelle aufgesetzt werden konnten. Steht nämlich eine Spitze schief auf der Haut, so wird die Empfindung einer Bewegung über die Haut hin erzeugt, die sehr leicht einen störenden Einfluss haben kann. Um dies zu vermeiden, wurden bei erheblicheren Distanzen die Schenkel des Zirkels immer entsprechend zusammengebogen. Die Spitzen selbst waren aus Knochen gefertigt und mit einer Breite des Berührungspunktes von annähernd $\frac{1}{2}$ mm abgerundet. Dies ist die beste Breite, weil sie einerseits allen Schmerz vermeidet, anderseits doch klein genug ist, um Maßbestimmungen von nur 2 mm zu ermöglichen. Dementsprechend wurden auch nur Hautstellen ausgewählt, bei denen kleinere Maßbestimmungen nicht erforderlich sind. Es darf nicht übersehen werden, dass die Form der Spitzen einen beträchtlichen Einfluss auf die Schwellenwerthe besitzt. Ohne Nadelspitzen hätte Goldscheider seine auffallend kleinen Schwellenwerthe nicht erhalten können. Bei unseren Versuchen wurde eine etwas größere Dicke genommen, theils weil die besonderen Erscheinungen, die untersucht werden sollten, von der Form der Spitzen ziemlich unabhängig sich erwiesen haben, theils weil größere Schwellenwerthe für die vorliegende Aufgabe im allgemeinen am günstigsten sind: wenn nämlich die Schwellenwerthe sehr klein sind, so bleiben die verschiedenen Abweichungen derselben unsicher, und dadurch können leicht bedeutende Täuschungen eintreten und dabei wichtige Erscheinungen übersehen werden.

Unsere erste Aufgabe war, wie bemerkt, den sogenannten Einfluss der Einübung auf das Erkennen minimaler Distanzen mittelst des Tastsinnes zu untersuchen. Die Forscher, die sich mit der Uebungsfrage beschäftigt haben, sind mit einander einverstanden, dass durch Uebung die Schwelle für die Wahrnehmung zweier Punkte verkleinert wird. Es erhebt sich dabei zugleich die Frage, ob, wenn eine Stelle eingeübt wird, dadurch andere Stellen miteingeübt werden. Darauf hat schon Volkmann geantwortet, dass, wenn eine von zwei symmetrischen Hautstellen eingeübt werde, die andere eine gleichmäßige Schwellenverkleinerung erfahre¹⁾.

1) »Ueber den Einfluss der Einübung auf das Erkennen räumlicher Distanzen«. Berichte d. sächs. Gesellsch. d. Wissensch. Bd. X (1858) math.-phys. Abth. S. 38 ff.

Funke¹⁾, der die Volkmann'schen Versuche erörtert, bemerkt, dass vielleicht eine allgemeine Schwellenverkleinerung auf der ganzen Hautoberfläche stattfindet, wogegen Dresslar²⁾ dies aus theoretischen Gründen zurückwies. Keiner von den Autoren, welche die Frage nach dem Umfang der Einübung untersucht haben, ist jedoch bei seinen Versuchen darauf specieller eingegangen. Die meisten scheinen anzunehmen, dass die Einübung nur eine Einübung der physiologischen Bedingungen des Ortssinnes der Haut selbst sei. Volkmann z. B. fand, dass durch Einübung der Volarseite des Zeigefingers einer Hand eine gleichmäßige Verkleinerung der Schwelle auf fünf anderen Stellen derselben und zugleich der andern Hand auftrat. Daraus schließt er, dass die zwei entsprechenden Nervenbahnen der beiden Arme, von denen jede eine gemeinsame Leitung zu den sechs Stellen darstellt, ein gemeinsames Centrum besitzen müssen, und dass die Einübung ein Vorgang in diesem Centrum sei. Nur einmal, und dies in neuester Zeit, nämlich von Judd³⁾, ist vermuthet worden, dass die Schwellenverkleinerung auf einen noch centraleren Einübungsprocess zurückzuführen sei.

Um diese Frage zu prüfen, wurden zunächst die Schwellen für zwölf bis zweiunddreißig Hautstellen bei jeder Versuchsperson ermittelt, sodann eine bestimmte bei verschiedenen Versuchspersonen verschiedene Stelle durch täglich vorgenommene Schwellenbestimmungen zwanzig oder dreißig Tage hindurch eingeübt. Zuletzt wurden die am Anfange auf verschiedenen Hautstellen festgestellten Schwellen wieder bestimmt und die Resultate mit den früheren Messungen verglichen, um zu sehen, ob sie größer, gleich oder kleiner geworden seien.

In allen diesen Versuchen wurde, wo es der Sache nach möglich war, die Methode der Minimaländerungen angewandt. Die Dauer des Aufsetzens der Spitzen wurde bei allen unseren Versuchen (abgesehen von einigen absichtlichen Veränderungen) möglichst constant gehalten, nämlich ungefähr 4 Secunden. Um dies zu erreichen, wurde in regelmäßigem langsamem Tact, für die Versuchspersonen unhörbar,

1) Hermann's Handbuch d. Physiologie, Bd. III, Abth. 2, S. 377—414.

2) American Journal of Psychology, Bd. VI (1894), S. 324—332.

3) Philos. Studien Bd. XI, S. 409 ff.

bis vier gezählt. Als Zwischenzeit für die einzelnen Versuche wurden in der Regel 10 oder 15 Secunden genommen. Zwischen den Reihen wurden häufige Pausen gemacht, um alle Ermüdung der Haut und der Aufmerksamkeit zu vermeiden. Von dieser Regel wurde eine Ausnahme gemacht, so oft die Versuchsperson um eine genauere Beschreibung ihres Verfahrens gebeten wurde. Alle Hautstellen (mit wenigen Ausnahmen) wurden mit Anilin markirt und die Lage des Körpertheils genau beschrieben, um dieselben Stellen bei denselben Lagen wieder zu finden; so wurde z. B. bei Schwellenbestimmungen auf der Hüfte zunächst ein Punkt markirt, von diesem aus wurden in zwei Richtungen die einzelnen Versuche gemacht, und sodann die weiteren Bedingungen der Versuche niedergeschrieben, ob z. B. die Versuchsperson stand oder auf einem Stuhl saß, ob im letzten Falle das Bein gleichfalls ausgestreckt auf einem Stuhl oder auf dem Boden ruhte u. s. w. Die Versuche wurden fast ausschließlich parallel der Längsachse des Körpers angestellt; eine Ausnahme wurde nur gemacht, um das Erkennen der Richtung oder die von Weber herrührende Behauptung, dass die transversalen Schwellen kürzer seien als die, welche der Längsachse des Körpers parallel sind, zu prüfen. Es wurden auch möglichst für jede Versuchsperson die allgemeineren Bedingungen der Versuche so constant wie möglich gehalten: nämlich die Temperatur des Zimmers¹⁾ und die Tageszeit. Ferner wurden die allgemeinen Körperzustände, wie Ermüdung, Kopfweh u. dergl. sorgfältig berücksichtigt. Uebrigens kam ich zu der Ueberzeugung, dass es im allgemeinen für die Zwecke dieser Versuche nutzlos sei, Essen, Schlafen und Arbeiten der Versuchsperson innerhalb gewisser sehr breiter Grenzen streng zu controliren. Es wurde nämlich bemerkt, dass die Gemüthslage der Versuchsperson eine sehr bedeutende Rolle spielt, während z. B. der Umstand, dass sie bis zwölf Uhr in der vorigen Nacht gearbeitet hatte, fast gar keine Rolle spielte. Der Einfluss solcher Bedingungen auf die Versuche hängt übrigens von der Individualität ab: es hat sich nur allgemein als die wichtigste Bedingung herausgestellt, dass die Aufmerksamkeit normal und ohne unwillkürliche Schwankungen sein muss.

1) Loewenton fand, dass die Temperatur des Zimmers einen erheblichen Einfluss auf die Schwelle ausübt. Versuche üb. d. Gedächtniss im Gebiete des Raumsinnes der Haut. Dorpat 1893. S. 18.

Es sei schon an dieser Stelle hervorgehoben, dass die Zeitdauer des Aufsetzens der Spitzen in der Regel auf das Urtheil keinen beträchtlichen Einfluss auszuüben scheint. Auf einige interessante Erscheinungen in dieser Hinsicht werden wir noch später zurückkommen. Nur dies sei bemerkt, dass, wenn die Versuchsperson wegen früherer Versuche oder Suggestion eine gewisse Dauer des Aufsetzens erwartete und die Spitzen nur für eine viel kürzere Zeit thatsächlich aufgesetzt wurden, sie entweder kein oder höchstens nur ein sehr unsicheres Urtheil abgeben kann. Im umgekehrten Falle gibt sie das Urtheil ab, bevor die Spitzen weggenommen werden; aber keine Veränderung aus Anlass der Fortdauer des Reizes wurde bei diesen Versuchen bemerkt. Wenn die Versuchsperson erwartet, dass die Spitzen nur für eine sehr kurze Zeit aufgesetzt werden, und ihre Erwartung thatsächlich erfüllt wird, so erhält man immer dieselben Resultate wie sonst. Die Länge der Zwischenzeit der Versuche kann aber zuweilen wegen starker Nachbilder, die bei einigen Versuchspersonen auftreten, und auch wegen der Spannung der Erwartung einen gewissen Einfluss haben. Um den Einfluss der Erwartung zu vermeiden, hat Camerer schon in ähnlichen Versuchen die Zwischenzeit sehr groß gewählt, fünf Minuten und in einer andern Reihe eine halbe Stunde. Die Erwartung bildet aber einen Theil der allgemeinen Frage nach dem Einfluss der Vorstellungen. Wenn die Versuchsperson schon vorher weiß, was für Versuche an ihr gemacht werden, kann eine längere Zwischenzeit den Einfluss der Erwartung gar nicht verändern: wenn dagegen die Versuchsperson von der Art der Versuche nichts vorher weiß, dann beruht die Erwartung auf einem vorhergehenden Urtheil, und die Beschaffenheit dieses Urtheils wird die Erwartung selbst verändern.

Eine weitere wichtige Bedingung dieser Versuche ist die Druckstärke der Spitzen. Um die inneren Bedingungen der Wahrnehmung zu constatiren, ist vor allem nothwendig, dass die zwei Spitzen mit gleicher Stärke in dem einzelnen Versuch und mit constanter Stärke in allen Versuchen, die nachher zur Vergleichung kommen, aufgesetzt werden. Um dieser Forderung nachzukommen wurde versucht, ein besonderes Aesthesiometer herzustellen, das die Stärke des Druckes bei jeder Lage des Körpertheils, auf dem die Versuche gemacht werden, genau misst. Außer den bekannten sehr beträchtlichen

mechanischen Schwierigkeiten gibt es jedoch auch andere Gründe, die dies nicht lohnend machen. Verschiedene Hautstellen besitzen nämlich für Druckempfindungen ganz verschiedene Empfindlichkeit, so dass dieselbe Druckstärke auf verschiedenen Hautstellen ganz verschiedene Empfindungsintensitäten erzeugt. So groß sind diese Abweichungen in der Sensibilität der Hautstellen, dass sehr oft in einem einzelnen Versuch die zwei Spitzen zwei Empfindungen von deutlich erkennbar verschiedenen Intensitäten erzeugten. Auf den Schulterblättern z. B. wurde versucht, gleiche Intensitäten von zwei in einer horizontalen Linie liegenden Empfindungen durch tiefes Drücken der inneren Spitze herzustellen. Bei diesem Versuch stellte sich folgendes heraus: Während die äußere Spitze ganz leise die Haut berührte und die innere so tief aufgedrückt wurde, dass sie die Haut fast zu verwunden schien, empfand die Versuchsperson die äußere Spitze bedeutend intensiver als die innere. Es ist von Külpe hervorgehoben, dass »eine reinliche Untersuchung der Raumschwelle die gleiche subjective Intensität überall herstellen muss«¹⁾. Doch werden die Empfindungen auf dem Rücken z. B. schmerzhaft, ehe sie dieselbe gleiche subjective Intensität erreicht haben wie die aus mittelstarken Berührungen entstehenden Empfindungen auf den Fingerspitzen oder auf der Zunge. Es wäre eine lohnende Aufgabe, die verschiedenen Empfindlichkeiten verschiedener Hautstellen für einfache Druckreize zu untersuchen. Bei der vorliegenden Arbeit aber glauben wir, dass die subjectiv scheinbare Intensität der Empfindungen eine viel geringere Rolle spielt als die Klarheit und Deutlichkeit derselben. Diese sind der Intensität der Empfindungen nicht durchaus proportional. Zwischen einem Reiz, der bloß empfunden wird, und einem Reiz, der schmerzhaft ist, gibt es auf jeder Hautstelle einen mittleren Reiz, der für die Wahrnehmung zweier Punkte am günstigsten ist, und dieser Reiz entspricht entschieden nicht bei jeder Hautstelle derselben äußeren Druckstärke. Wie wird nun dieser mittlere Reiz festgestellt? Er ist ebenso verschieden bei verschiedenen Hautstellen derselben Versuchsperson wie bei derselben Hautstelle verschiedener Versuchspersonen; und er ist bei derselben Hautstelle derselben Versuchsperson verschieden bei verschiedenen

1) Grundriss der Psychologie. Leipzig 1893. S. 352.

Anwendungen der Spitzen. Diese Thatsache lässt sich durch Aufmerksamkeitsschwankungen und körperliche Disposition der Versuchsperson leicht erklären. Es bleibt aber in Folge dessen nichts übrig, als den günstigsten Reiz während des Verlaufs der Versuche selbst rein empirisch festzustellen. Man darf wohl annehmen, dass im allgemeinen die auf diese Weise gewonnenen Schwellenwerthe viel werthbarer sind, als die durch einen complicirteren Apparat hergestellten. Damit es aber nicht nöthig würde, die Druckstärke jedesmal auszusuchen, wurde immer, wenn die Spitzen senkrecht aufgesetzt wurden, eine Stelle ausgesucht, wo die Haut ziemlich gleichmäßig empfindlich ist und wo das Gewicht des Zirkels, das 27,15 g betrug, bei jedem Versuch auf der Haut stehen gelassen werden konnte. Dies war das Verfahren in allen Versuchen auf dem Vorderarm, wo das Gewicht des Zirkels, also ungefähr 13,57 g auf jeder Spitze, für die Schwellenbestimmungen sich überall als sehr günstig erwies. Wo aber nur eine einzelne Spitze gebraucht wurde, wurde die günstigste Druckstärke erst durch einige Probeversuche festgestellt und dann möglichst constant gehalten. Bei einigen Versuchspersonen war es uns unmöglich, eine Stelle zu finden, wo die Strecke der annähernd gleichmäßigen Empfindlichkeit groß genug war, um dieses Verfahren reinlich durchzuführen. In solchen Fällen blieb nichts übrig, als die eine Spitze etwas stärker als die andere aufzusetzen: es wurde dabei immer darauf gesehen, dass der Versuchsperson die zwei Empfindungen immer von gleicher Intensität erschienen.

Andere Schwierigkeiten waren für diese Versuche die Schmerz- und Temperaturempfindungen. Solche Empfindungen waren immer als störende Factoren vorhanden, und ihr Einfluss konnte nur dadurch vermieden werden, dass die Versuchsperson beim Auftauchen dieser Empfindungen sofort Auskunft darüber gab. Es wurde zuerst versucht, die Temperatur- und Schmerzpunkte innerhalb jeder untersuchten Stelle auszusuchen und zu markiren, um ihre Berührung zu vermeiden. Es stellte sich aber heraus, dass die so gewonnenen Resultate in keinem Falle regelmäßiger waren als die Resultate derjenigen Versuche, in denen die Versuchsperson einfach über das Vorhandensein von Temperatur- und Schmerzempfindungen Auskunft gab. Uebrigens wirkte das Herausfinden solcher Punkte als eine Art

Einübung, welche die ersten Schwellenwerthe sehr klein machte. Um das Berühren von Haarpapillen zu vermeiden, wurde, wo solche Papillen vorhanden waren, ein Stück Haut rasirt.

Von den Resultaten der Untersuchungen sei hier einiges im allgemeinen vorausgeschickt. Es wurde von Weber behauptet, dass die Schwellenbestimmungen, die der Längsachse des Körpers parallel genommen werden, größer seien als die Bestimmungen, die rechtwinklig zur Längsachse gemacht werden. Für die Arme und Beine wurde diese Behauptung als ganz richtig befunden: für den übrigen Körper ergab sich aber aus den Versuchen, dass die Schwellenwerthe in den zwei Richtungen ziemlich dieselben für dieselbe Stelle waren. Wo Abweichungen von dieser Regel sich gezeigt haben, ist die transversale Schwelle eben so oft größer wie kleiner als die longitudinale. Auf dem Bauch z. B., der Brust, den Schulterblättern und dem Rücken fand sich in dieser Beziehung fast gar kein Unterschied. Auch die Angabe von Valentin¹⁾, dass die relativen Werthe der Schwellen für verschiedene Hautstellen annähernd gleich seien, können wir nur als eine sehr ungefähre Regel ansehen. Aus fünf Schwellenbestimmungen z. B., auf der rechten Hüfte, dem linken Vorderarm, dem rechten Oberarm, der linken Brust, dem rechten Daumen waren drei bei einer Versuchsperson größer als bei einer andern, während die andern zwei bei jener kleiner waren als bei dieser. Wenn man aber z. B. die relativen Werthe der Längs-Querschwellen auf dem Ober- und Unterarm bestimmt, so bleiben diese annähernd dieselben bei fast allen Versuchspersonen. Dasselbe gilt für die Beine. Für den Rumpf aber hat diese Regel fast gar keine sichere Geltung. Auch auf dem Kniegelenk konnten wir einen Unterschied zwischen der longitudinalen und der transversalen Schwelle nicht bemerken. Von Vierordt wurde das Folgende angegeben²⁾: »Die relative Feinheit des Ortssinnes eines bestimmten Hauptpunktes eines Körpertheils ist im Verhältniss zum Ortssinn der übrigen Punkte desselben Theils eine Function seiner Beweglichkeit, hängt ab von der relativen Größe der Excursionen, welche er bei den Bewegungen des betreffenden

1) Valentin, Lehrbuch der Physiologie des Menschen, Braunschweig 1844. Band II.

2) Pflüger's Archiv II, S. 297 und Vierordt's Grundriss der Physiologie 5. Aufl. S. 342.

Theils um die zugehörige Drehachse ausführt, wächst also proportional mit seinem Abstand von der Drehachse¹⁾. Wenn man die Extremitäten allein ins Auge fasst, so scheint sich diese Angabe zu bestätigen: die Fingerspitzen und die Zehen sind wohl am empfindlichsten. Im allgemeinen kann man aber annehmen, dass, je weiter man vom Fuß nach der Hüfte, oder von der Hand nach der Schulter geht, die Schwellenwerthe um so größer werden. Dabei erfährt die Schwelle eine bedeutende Zunahme in der Nähe jedes Gelenkes, und wenn man den Rumpf und den Kopf untersucht, bestätigt sich diese Regel überhaupt nicht mehr. Nach der Kritik, der Funke diese Angabe Vierordt's unterworfen hat, stellt sich heraus, dass man nicht nur die Größe der Excursion des Gliedes, sondern auch die Geschwindigkeit und Häufigkeit der Bewegungen des Körpertheils in Betracht ziehen muss. So führt er das Vierordt'sche Gesetz auf das allgemeine Uebungsgesetz zurück²⁾. Die Empfindlichkeit eines Körpertheils sei proportional dem Gebrauch des Theils beim Individuum und der durch Vererbung herübergenommenen Empfindlichkeitsanlage. Die Angabe Vierordt's könnte jedenfalls nur als ein rein empirisches Gesetz gelten: wenn es sich auch bestätigen ließe, so könnte es doch keineswegs die Erscheinungen erklären, die es zum Ausdruck bringt. In der That entspricht aber die Angabe nicht den Thatsachen.

Die Schwellenwerthe für die Wahrnehmung zweier Punkte haben sich in unsern Versuchen im allgemeinen viel inconstanter erwiesen, als nach den Angaben früherer Beobachter zu erwarten war. Dies führte uns zu der Ueberzeugung, dass jene Zahlenangaben zum Theil auf falschen Voraussetzungen beruhen müssten. Gerade die wohlbekannte Tabelle der Schwellenwerthe, die Weber festgestellt hat, darf nicht als allgemein gültig angesehen werden. Man darf wohl annehmen, dass es nirgends in der Welt einen Menschen gibt, bei dem alle in dieser Tabelle angegebenen Schwellenwerthe bestätigt werden können. Wo man einige Schwellenwerthe von derselben Größe wie die von Weber angegebenen findet, werden andere ganz beträchtlich davon abweichen. Uebrigens glauben wir nach

1) Diese Formulirung ist aus Funke-Hermann's Handbuch a. a. O.

2) Seit Funke hat auch Klingenberg dasselbe gethan: Der Raumsinn der Haut und seine Modification durch äußere Tastreize. Bonn, 1883. S. 20.

unserer Erfahrung nicht, dass die Schwellenwerthe für dieselben Hautstellen bei derselben Versuchsperson z. B. innerhalb 0,5 mm in je zwei successiven Bestimmungen genau dieselben bleiben. Manchmal erreichen die Variationen des Schwellenwerthes für dieselbe Hautstelle z. B. am Rumpf, Oberarm oder Oberschenkel eine Größe von 5 bis 10 mm. Wir haben es versucht, durch eine große Anzahl von Schwellenbestimmungen, deren Durchschnitt als die Schwelle angenommen wurde, die Weber'schen Schwellenwerthe herzustellen. Aus diesem Versuch ergab sich, dass die Schwelle in einzelnen Bestimmungen verschieden ist je nach der Form der angewandten Spitzen, und je nachdem man als Schwelle die erste Distanz, die bei dem aufsteigenden Verfahren erkannt wird, annimmt, oder diejenige, wo das Urtheil nicht mehr schwankend ist, oder endlich das Mittel zwischen jener und dieser. Bei jeder einzelnen Bestimmung konnten wir die Weber'sche Zahl annähernd, d. h. innerhalb 3 mm herstellen, wenn wir uns offen hielten, irgend eine von den drei Bestimmungsweisen zu wählen. Wenn wir aber eine von den drei Arten von vornherein auswählten und als die Schwelle in allen einzelnen Bestimmungen constant festhielten, um den Durchschnitt zu ziehen, so erhielten wir fast niemals den Weber'schen Schwellenwerth. Die von Weber angegebenen Schwellenwerthe können wir daher nur als ungefähre ansehen, was sich wohl daraus erklärt, dass bei der von Weber angewandten Methode der »eben bemerklichen Unterschiede« die Schwellenbestimmung nicht mit der gleichen Regelmäßigkeit geschieht wie bei der von uns angewandten Methode der Minimaländerungen.

I.

Wie oben angegeben, wurden die vorliegenden Untersuchungen ursprünglich von zwei Experimentatoren begonnen, denen natürlich das Programm derselben durchweg bewusst war. In den Vorversuchen, die wir zusammen ausführten, wurde die Methode der Minimaländerungen unmodificirt gebraucht. Bei jedem Versuch wurde mit dem Aufsetzen bloß eines Punktes angefangen, dann mit zwei Punkten, von minimalen Distanzen ausgehend, in Abstufungen fortgeföhren, bis die Distanz deutlich über der Schwelle war; dann wurde die Distanz ebenso regelmäßig wieder vermindert, bis die Spitzen nicht mehr als zwei erkannt werden konnten. Bei dem aufsteigenden Ver-

fahren wurde die Distanz gewonnen, bei welcher die Spitzen zuerst als zwei sicher erkannt wurden, bei dem absteigenden diejenige, bei welcher sie als zwei zum letzten Male erkannt wurden. Von diesen zwei Zahlen wurde das arithmetische Mittel als die Schwelle genommen. Jeder Schwellenwerth wurde in dieser Weise zweimal bestimmt und demnach das arithmetische Mittel aus vier Bestimmungen, zwei aufsteigend, zwei absteigend, schließlich als die Schwelle angenommen. In der Regel blieben die Spitzen auf der Haut, bis die Versuchsperson antwortete. Die Zwischenzeit der einzelnen Versuche war stets 20 Secunden. Die Druckstärke war bei den Versuchen nicht durchweg gleichmäßig. Die Tageszeit war immer dieselbe, nämlich 8 Uhr Vormittags. Nachdem wir die Schwellen auf 10 Hautstellen bei uns beiden bestimmt hatten, wählten wir eine Stelle für die tägliche Uebung aus. Es wird von Verschiedenen, z. B. von Funke, angegeben, dass die Schwellenverkleinerung schließlich auf einen minimalen Werth führe, bei welchem die Schwelle constant bleibe. Um diesen Grenzwert zu erreichen, wurden die täglichen Bestimmungen 30 Tage lang fortgesetzt. Jene Erwartung wurde aber nicht erfüllt. Von Anfang an waren die Schwellenwerthe fast immer verschieden, indem die absteigend bestimmten gewöhnlich kleiner waren als die aufsteigend bestimmten — eine Erscheinung, die schon seit lange bekannt ist¹⁾. Von Tag zu Tag wurde aber der Unterschied der zwei Werthe ein größerer, bis am Ende der Reihen bei den Bestimmungen von oben nach unten keine sichere Schwelle mehr bestimmt werden konnte: die Versuchsperson antwortete nämlich in diesen Fällen »zwei Spitzen«, nachdem die frühere aufsteigend bestimmte Schwelle längst vorbei war. Auch antwortete die Versuchsperson ziemlich oft »zwei Spitzen«, wenn nur eine aufgesetzt war — eine Erfahrung, die im Anfange gar nicht vorkam. Schließlich wurden diese Vexirfehler (wie sie Fechner genannt hat) so häufig, dass bei der Wiederholung der sechs Schwellenbestimmungen auf verschiedenen Hautstellen in der Regel keine sicheren Schwellen mehr ermittelt werden konnten. Vor dem Aufhören der täglichen Uebungen war die Schwelle auf der linken Kniescheibe der einen Versuchsperson von 11 mm bis auf 3 mm hinuntergegangen, und die Schwelle auf dem

1) Wundt, Beiträge zur Theorie der Sinneswahrnehmung. Leipzig und Heidelberg 1862. S. 43.

linken Handgelenk der andern Versuchsperson von 19 mm bis auf 7. Diesen kleinen Werthen entsprechend schien es, als ob die andern sechs Werthe auf den nicht eingeübten Stellen eine ebenso beträchtliche Verminderung gezeigt hätten. Doch machten die Vexirfehler, die schließlich bei beiden Verfahrensweisen erschienen, diese Bestimmungen ganz unzuverlässig. Es war daher aus diesen Versuchen nur zu schließen, dass das Resultat der Einübung wahrscheinlich ein allgemeines ist. Die scheinbare Verkleinerung der Schwellen und die Vexirfehler selbst konnten möglicher Weise durch irgend eine Unregelmäßigkeit in den Versuchsbedingungen vorgekommen sein. Es blieb daher nichts übrig als die Versuche noch einmal unter constanteren Bedingungen zu beginnen. Immerhin ergaben sich aus diesen Versuchen neue Fragen. Die scheinbare Schwellenverkleinerung schien nicht überall gleichmäßig zu sein. Die Vexirfehler ferner kamen auf verschiedenen Hautstellen in sehr ungleichem Maße zum Vorschein. Ungefähr gleichzeitig mit dem Vexirfehler schienen schließlich in eigenthümlicher Weise zwei Schwellen vorhanden zu sein, eine kleinere und eine größere mit einer Zwischenstrecke, wo nur ein Punkt wahrgenommen wurde. Weil wegen der häufigen Vexirfehler sehr oft keine Bestimmung der Schwelle in absteigender Richtung möglich war, wurde im Folgenden lediglich das aufsteigende Verfahren angewandt. Anstatt die Schwelle zweimal von unten nach oben und zweimal von oben nach unten zu bestimmen, wurden nun jedes Mal vier Bestimmungen von unten nach oben gemacht und das arithmetische Mittel aus ihnen genommen. In allen folgenden Versuchen wurde auch die Zeitdauer des Reizes auf 4 Secunden, wie schon beschrieben, beschränkt. Die Stellen wurden markirt und anstatt den Zirkel mit unregelmäßiger Druckstärke auf die eingeübte Stelle aufzusetzen, wurde auch noch dieser Factor völlig constant gehalten. Die eingeübte Stelle war, außer bei Herrn Arrer, bei jeder Versuchsperson auf dem Vorderarm, wo der Zirkel senkrecht aufgesetzt werden konnte. Es wurde eine Stelle ausgesucht, wo gerade das Gewicht des Zirkels für die Bestimmungen günstig erschien. Bei Herrn Arrer war die eingeübte Stelle in der Mitte des rechten Oberarmes, und auf dieser Stelle musste man sich begnügen, die Spitzen so regelmäßig wie möglich aufzusetzen: dabei waren nur solche empirische Maßstäbe möglich, wie das beim Aufsetzen der Spitzen statt-

findende Einsinken der Haut, die Widerstandsempfindung des ausgeübten Druckes u. s. w., wonach die Druckstärke der Spitzen gemessen werden konnte. Es sei aber hervorgehoben, dass wir uns fortwährend durch die Aussagen der Versuchsperson in allen Versuchsreihen versichert haben, dass die Druckstärke angemessen sei. Dazu kommt noch, dass die günstigen Bedingungen viel leichter herzustellen sind, als man beim ersten Blick anzunehmen geneigt ist: man gewinnt nämlich sehr leicht eine große Fertigkeit in diesem Verfahren, wovon jeder sich überzeugen kann, wenn er einige Versuchsreihen ausführt. Um die Bedingungen der Volkmann'schen und Drësslar'schen Versuche völlig herzustellen, wurden der Versuchsperson im Anfange der Zwecke der Versuche sowie die bis dahin gewonnenen Resultate mitgetheilt. Allen Versuchspersonen, außer Herrn Perry, wurde auch ausdrücklich gesagt, dass die Resultate gar nicht verwerthbar sein würden, im Falle Vexirfehler vorkämen; ferner wurde die Versuchsperson gebeten, niemals zwei zu antworten, bis sie völlig sicher sei. Versuchspersonen waren die Herren Hefelbower (Hef.), Arrer (A.) und Perry (P.). —

Unter diesen Versuchen sind die des Herrn Hef. außerordentlich frei von Vexirfehlern. Wir können sagen, dass diese bis gegen das Ende der Reihe fast gar nicht zum Vorschein kamen. Die Reihe wurde nur 20 Tage weiter geführt, nicht 30, wie bei den Vorversuchen. Die Tageszeit war immer dieselbe, $\frac{1}{2}$ 7 Uhr morgens. Die Tabellen I und II zeigen die zwei Reihen von Schwellenbestimmungen auf mehreren Hautstellen, die vor und nach der Einübungsreihe gemacht wurden. Da Herr Hef. sehr gesund und eine gute Versuchsperson ist, so suchte ich die Zahl der Hautstellen zu vermehren, um zu sehen, ob dieselbe Verkleinerung der Schwellen auf der linken wie auf der rechten Seite des Körpers stattfindet. Es wurden diese Stellen auch nach der Unterlage der Hautstelle, ihrer Lage am Körper und ihrer Entfernung von der Drehachse des Gliedes ausgewählt. Es war nämlich möglich, dass es einen Unterschied machte, ob die Unterlage Knochen oder Fleisch, ob die Haut selbst dick oder dünn, rauh oder glatt war, und ob die Stelle nahe oder fern von der Drehachse des Gliedes lag. Aber kein solcher Einfluss der Unterlage oder des Zustandes der Hautstelle oder ihrer Entfernung von der Drehachse ist in den Tabellen zu sehen.

Tabelle I.
Beob.: Hef. 16 Schwellen auf der linken Seite.

Datum	Eingübte Stelle Vorderarm dorsal	Handrücken	Brust	Kniescheibe	Hüfte	Schulsselbein	Oberarm	Vorderarm	Äußerer Knöchel	Dammen	Innerer Knöchel	Bauch	Oberschenkel	Backen	Ferse	Oberer Theil des Fusses
Juni 28.—30.	50	25	37	30	70	48	70	48	36	13	38	41	60	13	17	33
Juli 20.—23.	5	4	6	8	20	14	24	20	13	5	16	18	23	12	11	20
Relativer Werth der Einübung	50 : 5 10	25 : 4 6 $\frac{1}{4}$	37 : 6 6 $\frac{1}{6}$	30 : 8 3 $\frac{3}{4}$	70 : 20 3 $\frac{1}{2}$	48 : 14 3 $\frac{3}{7}$	70 : 24 2 $\frac{11}{12}$	48 : 20 2 $\frac{4}{5}$	36 : 13 2 $\frac{10}{13}$	13 : 5 2 $\frac{3}{5}$	38 : 16 2 $\frac{3}{8}$	41 : 18 2 $\frac{5}{18}$	60 : 23 2 $\frac{14}{23}$	13 : 12 1 $\frac{1}{12}$	17 : 11 1 $\frac{6}{11}$	33 : 20 1 $\frac{13}{20}$

Drei Schwellen in der Mittellinie des Körpers.

Datum	Rücken zwischen den Schultern	Rücken zwischen den Hüften.	Stirn
Juni 28.—30.	55	57	22
Juli 20.—23.	30	28	13
Relativer Werth der Einübung	55 : 30 1 $\frac{5}{6}$	57 : 28 2 $\frac{1}{28}$	22 : 13 1 $\frac{9}{13}$

Tabelle II.
 Beob.: Hef. 16 Schwellen auf der rechten Seite.

Datum	Symmetrische Stelle	Handrücken	Brust	Kniekehle	Hüfte	Schulasselbein	Oberarm	Vorderarm	Außere Knochel	Daumen	Innere Knochel	Bauch	Oberschenkel	Backen	Ferse	Oberer Theil des Fußes
Juni 28.—30.	50	25	50	18	88	25	70	58	55	12	48	27	70	13	15	30
Juli 20.—23.	5	11	5	10	19	10	33	24	15	6	14	13	12	10	8	15
Relative Werth	$\frac{50:5}{10}$	$\frac{25:11}{23/11}$	$\frac{50:5}{10}$	$\frac{18:10}{14/5}$	$\frac{88:19}{412/19}$	$\frac{25:10}{21/2}$	$\frac{70:33}{24/33}$	$\frac{58:24}{25/12}$	$\frac{55:15}{32/3}$	$\frac{12:6}{2}$	$\frac{48:14}{38/7}$	$\frac{27:13}{21/13}$	$\frac{70:12}{55/6}$	$\frac{13:10}{13/10}$	$\frac{15:8}{17/8}$	$\frac{30:15}{2}$

Aus diesen Tabellen könnte man, nach dem Vorgang Volkmann's, schließen, dass die Einübung kein peripherer, sondern ein centraler Vorgang irgend welcher Art sei. Volkmann hat nämlich gefunden, dass bei der Einübung der Volarseite des Endgliedes des Zeigefingers fünf andere Stellen auf der Hand mit eingeübt werden. Daraus schließt er, dass diese sechs Stellen eine gemeinsame Nervenbahn besitzen. Da die andere Hand eine gleichmäßige Miteinübung erfuhr, so schließt er, dass die zwei Bahnen von den zwei Händen ein gemeinsames Centrum haben. Hätte er nun noch mehr Hautstellen geprüft, so hätte er schließen müssen, dass das ganze Centrum eingeübt würde. Auf diese Frage werden wir später noch genauer eingehen. Die Schwellenverkleinerungen auf der eingeübten und der ihr symmetrischen Stelle sind hier verschieden; auch sind die andern Verkleinerungen ungleichmäßig, und insofern werden die weniger sicheren Resultate der Vorversuche bestätigt.

Tabelle III, nebst der Curve Fig. 1, zeigen die Verkleinerung auf der eingeübten Stelle. Die Regelmäßigkeit der Abnahme ist dadurch zu erklären, dass bei jedem Experiment die Schwelle viermal bestimmt und der Durchschnitt als die Schwelle für den Tag angenommen wurde. Dasselbe Verfahren wurde auch bei den Herren A. und P. gebraucht, aber nicht mit so regelmäßigen Resultaten. Man könnte vermuthen, dass Herr Hef. die einzelnen Abstufungen gezählt oder durch irgend ein anderes Hülfsmittel sein Urtheil gewonnen habe. Es wurde jedoch alles versucht, Vermehrung oder Verminderung der Zahl der Abstufungen, Einschlebung von Vexirversuchen u. s. w., um seine Urtheile zu prüfen. Es wurden z. B., wenn die Schwelle nur 5 mm zu sein schien, bald 5 bald 10 Abstufungen angewandt, ehe die Entfernung der Spitzen 5 mm wurde; und dabei wurden Reihen von z. B. 10 Vexirversuchen, wo nur eine Spitze aufgesetzt war, eingeschoben. Dennoch hat Herr Hef. die in den Tabellen als Schwellen angegebenen Entfernungen, einige Vexirfehler ausgenommen, außerordentlich regelmäßig angegeben.

Tabelle III.

Beob.: Hef. Abnahme der Schwelle auf dem linken Vorderarm durch tägliche Uebung vom 1. bis 20. Juli.

Datum	VII. 1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.
Schwelle	50	35	29	24	23	18	18	14	14	12	12	12	11	11	7	7	5	7	4	5



Fig. 1.

Aus dieser Tabelle ist zu sehen, dass in den ersten 7 Tagen die Schwelle schon um 32 mm, innerhalb der ganzen 20 Tage aber nur um 45 mm abgenommen hat. Im Anfange der Versuche schien Herr Hef. ganz unfähig, Vexirfehler zu machen; eine Spitze nahm er immer richtig wahr. Am Ende der Versuche erschienen im Gegentheil Vexirfehler mit größerer oder geringerer Häufigkeit überall auf allen Hautstellen; an einem Tage waren sie sehr häufig. Am 10. Juli erschienen Vexirfehler zum ersten Male, und an demselben Tage kamen auch die früher bemerkten zwei Schwellen zum Vorschein. Von Tag zu Tag aber verschwand die obere Schwelle allmählich, und nur die untere, die immer kleiner wurde, blieb. Der Vexirfehler verschwand aber nicht mit der oberen Schwelle.

Bei Herrn Arrer waren Vexirfehler am Anfang sehr selten, obwohl nicht ganz ausgeschlossen. Seine Schwellen waren immer deutlich und relativ groß. Da Herr A. ein geübter und sehr guter Beobachter ist, dürfen wir dieser Reihe einen hohen Werth zuschreiben. Tabelle IV zeigt die Schwellen, welche vor und nach der Einübungsreihe gefunden wurden. Die eingeübte Stelle war auf der Dorsalseite des rechten Oberarms.

In dieser Tabelle sehen wir, wie in Tabelle I und II, die Verkleinerung auf der geübten und der ihr symmetrischen Stelle im allgemeinen größer als die Verkleinerungen auf andern Stellen. Im allgemeinen ist die Abnahme der Schwellen entschieden geringer als in Tabelle I und II, vielleicht weil Herr A. von Anfang an kleinere Schwellen hatte. Die vor der Einübung gewonnenen Schwellenwerthe bei Hef. und A. waren von einem Viertel bis zur Hälfte verschieden — Variationen, die Valentin schon bemerkt hat. Das Vierordt'sche »Gesetz« wird in den vorliegenden Tabellen nur in den entferntesten Umrissen bestätigt und es finden sich viele Ausnahmen. Auch die Verhältnisse der Schwellenverkleinerungen bei Hef. und A. sind keineswegs übereinstimmend. Tabelle V und die Curven Fig. 2 zeigen die Abnahme der Schwellen auf der geübten Stelle, dem rechten Oberarm, bei A. Man sieht in der Curve große Unregelmäßigkeiten. Sie sind theilweise dadurch zu erklären, dass die Versuche oft für einen oder zwei Tage unterbrochen wurden, theilweise durch Schwankungen der Aufmerksamkeit wegen Ermüdung, und theilweise wahrscheinlich durch Einflüsse, die noch nicht nachgewiesen sind.

Tabelle IV.
Beob.: A. 19 Schwellen.

Datum	Eingetübte Stelle Oberarm dorsal rechts	Symmetrische Stelle links	Vorderarm rechts	linker Schenkel	links Hüfte	rechts Stirn	links Handrücken	links Bauch	links Fuß	links Knie	rechts Schulterblatt	links Schlüsselbein	links Handvolar	links Knochel außen	links Knochel innen	rechts Brust	links Ferse	Mitte des Rückens	links Wangen
Juli 1.-7.	55	66	30	50	25	25	20	60	37	22	55	54	8	41	22	45	18	40	13
August 1.-6.	5	7	10	20	10	10	8	26	16	10	25	25	4	22	12	25	13	25	12
Relativer Werth der Einübung	$\frac{55:5}{11}$	$\frac{66:7}{39/4}$	$\frac{30:10}{3}$	$\frac{50:20}{21/2}$	$\frac{25:10}{21/2}$	$\frac{25:10}{21/2}$	$\frac{20:8}{21/2}$	$\frac{60:26}{24/13}$	$\frac{37:16}{25/16}$	$\frac{22:10}{21/5}$	$\frac{55:25}{21/5}$	$\frac{54:25}{24/25}$	$\frac{8:4}{2}$	$\frac{41:22}{119/22}$	$\frac{22:12}{15/6}$	$\frac{45:25}{14/5}$	$\frac{18:13}{15/13}$	$\frac{40:25}{13/5}$	$\frac{13:12}{11/12}$

Tabelle V.

Beob.: A. Abnahme der Schwelle auf dem rechten Oberarm, zuletzt (vom 25. an) zwei Schwellen.

Datum	VII. 8.	10.	11.	13.	15.	16.	17.	19.	20.	22.	23.	24.	25.	26.	29.	30.	31.	VIII. 1.	3.	4.
Schwellen	55	40	46	52	38	34	37	24	19	28	33	11	5	7	8	4	5	7	6	10
													28	30	30	15	15	27	—	23

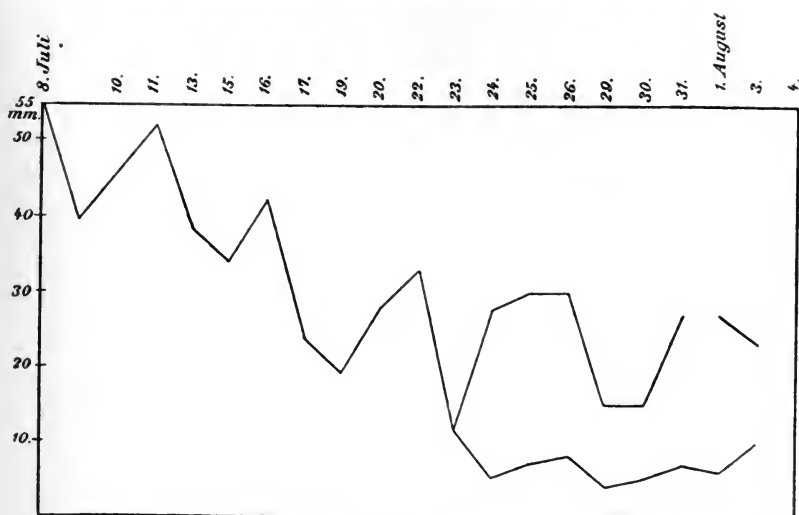


Fig. 2.

Nach dem elften Tage sind die zwei Schwellen, von einem Tage abgesehen, immer vorhanden gewesen. Vexirfehler waren bei A. entschieden häufiger am Ende der Reihe als am Anfange. An einigen Tagen erschienen sie auch häufiger als an anderen. Ferner waren sie bedeutend häufiger in der Nähe der unteren Schwelle als der oberen.

Es schien nun wünschenswerth, um die allgemeine Verkleinerung der Schwelle festzustellen, eine Reihe von Versuchen auszuführen, bei denen zwar Vexirfehler, aber doch in geringer Zahl vorkommen. Demgemäß wurde der Versuchsperson zunächst gar keine Auskunft über Vexirfehler gegeben, und damit sie nichts von der Empfindung einer einzelnen Spitze erfahren konnte, wurde niemals eine Spitze allein auf die Hand aufgesetzt¹⁾. Hierher gehört die Reihe bei Herrn Perry. Ihm wurde am Anfange alles über die Versuche gesagt, aber nichts über das Vorkommen von Vexirfehlern; dabei wurde durchaus vermieden, mit einer Spitze allein die Haut zu berühren. Außerdem war das Verfahren wie bei A. und Hef. Die Dauer des Aufsetzens war immer annähernd 4 Secunden mit regelmäßigen Zwischenzeiten von 15 Secunden. Die Tageszeit wurde nicht durchweg constant gehalten. Die meisten Versuche wurden gegen 2 Uhr,

1) Dieses Verfahren verdanken wir der Güte des Herrn Prof. Wundt.

gerade nach dem Mittagessen, ausgeführt; aber einige fanden später am Nachmittag und einige auch früher am Vormittag statt, denn es schien besser, die Tageszeit etwas zu variiren, als die Versuche für einen oder zwei Tage ganz ausfallen zu lassen. Die Versuche, die Vormittags ausgeführt wurden, wurden in dem Zimmer des Herrn P. gemacht, die andern in dem Institute. Wie alle anderen Versuchspersonen machte Herr P. beim Aufsetzen der Spitzen die Augen zu, und es wurde außerdem dafür gesorgt, dass er während der Stunde nicht nach der untersuchten Stelle hinschaute. Tabelle VI gibt die Schwellenwerthe an, die vor und nach der Einübungsreihe erhalten wurden. Die Einübung wurde wegen unvermeidlicher Unterbrechung nur 15 Tage lang fortgesetzt. Die eingeübte Stelle war auf der Volarseite des rechten Vorderarms.

Tabelle VI.

Beob. P. Schwelle an sechs Stellen der Haut.

Datum	Eingeübte Stelle rechter Vorderarm	Symme- trische Stelle l. Vorderarm	rechter Oberarm	linker Oberarm	rechtes Bein	linkes Bein
XI. 26.—29.	40	42	68	54	43	45
XII. 17.—20.	15	14	24	24	22	18
Relativer Werth der Einübung	$\frac{40 : 15}{2\frac{2}{3}}$	$\frac{42 : 14}{3}$	$\frac{68 : 24}{2\frac{5}{6}}$	$\frac{54 : 24}{2\frac{1}{4}}$	$\frac{43 : 22}{2}$	$\frac{45 : 18}{3}$

Diese Tabelle zeigt, dass die allgemeinen Schwellenverkleinerungen etwas gleichmäßiger sind, als bei A. und Hef., vielleicht weil die Hautstellen überhaupt hier kleiner an Zahl sind. Merkwürdigerweise ist die scheinbare Einübung der zur eingeübten symmetrischen Stelle und der Stelle auf dem linken Bein größer als die der eingeübten Stelle selbst: also wird eins der Resultate der Versuche bei Hef. in dieser Tabelle nicht bestätigt, sondern ihm geradezu widersprochen. Vexirfehler waren bei P. etwas häufiger als bei Hef. und nicht ganz so häufig als bei A. Unter »Vexirfehler« verstehen wir hier die Antwort »zwei Spitzen«, wenn die Entfernung der Spitzen thatsächlich weit unter der angegebenen Schwelle liegt, z. B. wenn auf dem rechten

Oberarm, wo die Schwelle 68 mm ist, bei einer Entfernung der Spitzen von 5 mm »zwei Spitzen« geantwortet wurde, nachdem schon eine Entfernung von 20 mm nicht als zwei Spitzen erkannt worden war. Aus der Erfahrung an anderen Versuchspersonen kann man schließen, dass, wenn man eine Spitze aufgesetzt hätte, ungefähr dieselbe Zahl von Vexirfehlern herausgekommen wäre, als beim Aufsetzen von zwei Spitzen bei einer Entfernung von z. B. 4 oder 5 mm. Es wurden hier nur diejenigen Antworten den Vexirfehlern zugeschrieben, die beim Aufsetzen der Spitze bei den kleinsten Entfernungen, die überhaupt angewandt wurden, vorkamen. Aus Tabelle VII und der Curve Fig. 3 sieht man, dass P. außer diesen Vexirfehlern noch zwei deutliche Schwellen ziemlich oft angegeben hat. In der That kamen diese zwei Schwellen und die Vexirfehler ziemlich gleichzeitig zum Vorschein. Außerdem vermehren sich die Vexirfehler nach einem gewissen Punkt proportional der Verkleinerung der Schwelle, eben wie die Vexirfehler, die beim Aufsetzen einer einzelnen Spitze in den früheren Tabellen vorkommen. Tabelle VII gibt die Einübungsbestimmungen an, die auf der Volarseite des linken Vorderarmes gemacht wurden.

Tabelle VII.

Beob.: P. Abnahme der Schwelle am linken Vorderarm in 15 Tagen.

Datum	XI. 26.	30.	XII. 2.	3.	4.	5.	6.	7.	9.	10.	11.	12.	13.	14.	17.
linker Vorderarm	40	26	21	23 19	28	25 16	23 17	20	19 12	16 11	26 9	20 10 ¹ / ₃	24 7	23 11	15

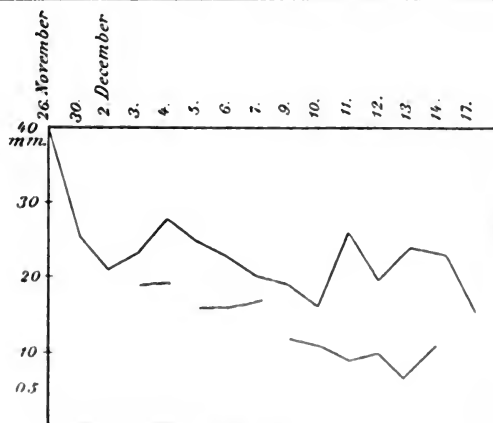


Fig. 3.

Auch bei anderen Versuchspersonen, deren Resultate wegen besonderer Erscheinungen, die bei ihnen vorgekommen sind, hier noch nicht angeführt werden können, stellte sich heraus, dass das Vorkommen von Vexirfehlern von dem Wissen über dieselben und von dem Aufsetzen nur einer Spitze unabhängig erscheint. Es deutet dies auf eine andere Erklärung des Vexirfehlers hin, als sie bis jetzt gegeben wurde. Hier sei nur hervorgehoben, dass Herr P. von Anfang bis zum Ende dieser Versuche von Vexirfehlern überhaupt selbst nichts wusste. Im Anfang waren seine Antworten durchaus eindeutig: die kleineren Entfernungen der Spitzen erkannte er immer als eine Spitze an, obwohl er von Anfang an wusste, dass eine Spitze in diesem Versuche nicht angewandt wurde. Am Ende dieser Versuche wurde absichtlich nur eine Spitze angewandt, um zu erfahren, ob er die eigentlichen Vexirfehler machen werde. Das Resultat war überraschend eindeutig: auf dem Vorderarm, wo die Schwelle im Anfange sehr deutlich 40 mm war, nahm er eine Spitze als »zwei« 10mal nacheinander wahr. Beim elften Aufsetzen der Spitze antwortete er »eins«, aber bei den hier gemachten 25 Versuchen zusammen nahm er die Spitze 19mal falsch wahr.

Es ist aus den bis jetzt angeführten Tabellen ersichtlich, dass die von Volkmann, Fechner, Funke und Dresslar beobachtete Erscheinung eine Verkleinerung der Schwelle für die Wahrnehmung zweier Punkte (von Volkmann und Dresslar »eine Verkleinerung der Raumschwelle« und von Funke »eine Verfeinerung des Ortsinnes der Haut« genannt) in der That auf die eingeübte und die ihr symmetrische Hautstelle nicht beschränkt ist. Dies war schon aus den Vorversuchen wahrscheinlich; aus den Versuchen bei Herrn Hef. bei im ganzen 32 verschiedenen Hautstellen ist der Schluss augenfällig. Nur an einer Stelle bei dieser Versuchsperson gab es fast keine Verkleinerung der Schwelle: nämlich auf der linken Backe unter dem Auge war die Schwelle 12 mm gegen 13 mm am Anfange. Dies ist aus den allgemeinen Gründen, die wir später anführen wollen, wohl erklärbar. Die Resultate der Versuche bei A. und P. sind mit diesem allgemeinen Schluss durchaus übereinstimmend. Von Dresslar¹⁾ wird angegeben, dass ein Unterschied in der Qualität

1) a. a. O. S. 329.

der Empfindung bei Berührung eines Punktes innerhalb und außerhalb der eingeübten Strecke der Haut nach der Einübungsreihe zu erkennen war. Die Unterschiede der Localzeichen verschiedener Hautpunkte gingen, wie es scheine, allmählich in einander über; nach der Einübung aber seien diese Uebergänge zwischen den Punkten innerhalb der eingeübten Strecke und denjenigen, die unmittelbar außerhalb derselben liegen, nicht mehr vorhanden, sondern es geschehe ein plötzlicher Sprung von einer Stelle zur anderen. Die Punkte, die innerhalb dieser Strecke liegen, seien von den umgrenzenden Punkten qualitativ verschieden. Dadurch könne die Versuchsperson angeben, ob ein berührter Punkt innerhalb oder außerhalb der eingeübten Strecke liege. Daraus schloss er, dass die Endorgane eine Veränderung, nicht im Bau, sondern in der Function erfahren hätten. Diesen angeblichen »Qualitätsunterschied« konnten wir jedoch bei keiner Versuchsperson finden. Die Versuchsperson kann wohl in der Regel die eingeübte Hautstrecke ungefähr abgrenzen, aber einfach dadurch, dass sie die Localzeichen der Punkte innerhalb derselben, die so oft im Bewusstsein während der Einübung hervorgerufen wurden, wiedererkennt. Außerdem giebt Dresslar an, dass die Schwellenwerthe, die in der unmittelbaren Nachbarschaft der eingeübten Hautstrecke gefunden wurden, viel grösser waren als die an der eingeübten Hautstrecke selbst. Diese Angabe stimmt erstens mit den Resultaten Volkmann's¹⁾, der dieselbe Verkleinerung auf fünf anderen Stellen derselben Hand fand, nicht überein. Zweitens könnte diese Angabe über die Frage nach einer allgemeinen Verkleinerung der Schwellen für die Wahrnehmung zweier Punkte nur in dem Falle entscheidend sein, wenn der Verfasser außerdem bewiesen hätte, dass die Erscheinung nicht auf andere Art (z. B. durch Suggestion) erklärbar sei. Hierauf werden wir noch zurückkommen. Uebrigens waren die Hautstrecken, die Dresslar einüben wollte, ziemlich groß, nämlich bei einer Versuchsperson 5 cm im Quadrat und bei der andern 7 cm im Quadrat. Es ist also nicht leicht zu denken, dass alle die Druckpunkte dieser Strecken innerhalb eines Monats functionelle Verschiedenheiten in gleichem Maße zeigen sollten: es müssten daher irgend welche Unterschiede zwischen diesen

1) a. a. O.

Punkten selbst immer noch vorhanden gewesen sein, auch wenn man die Annahme von Dresslar wahrscheinlich fände.

Wie werden wir aber dann eine solche Einübung erklären? Es wird von Dresslar hervorgehoben, dass von einer Veränderung im Bau des Tastorganes während der kurzen Zeit dieser Versuche keine Rede sein könne; und er findet es deshalb wahrscheinlicher, dass sich eine Veränderung in der Function der Tastorgane vollziehe. Da jedoch dieser Schluss nur aus den Versuchen an zwei symmetrischen Stellen gezogen wurde, so ist er nicht ohne weiteres anzunehmen. Es ist sodann in neuester Zeit versucht worden, die Einübung als ein rein psychisches Phänomen zu betrachten und auf eine Verstärkung der Aufmerksamkeit zurückzuführen¹⁾. Hierbei ist jedoch die Thatsache nicht berücksichtigt, dass die Einübung zugleich zu Trugwahrnehmungen führt: eine solche ist offenbar der Vexirfehler, den keine Verstärkung oder Verringerung der Aufmerksamkeit zu erklären vermag.

II.

Die nunmehr folgenden Beobachtungen wurden an einer Anzahl von Versuchspersonen gewonnen, die überhaupt keine sichere Schwelle zu haben schienen, wobei aber nur schwer eine solche zu finden war, weil sie fortwährend Vexirfehler machten, so dass auch die Einübung bei ihnen schwer zu verfolgen war. Diese Versuchspersonen lassen sich je nach der Häufigkeit der Vexirfehler in drei Classen ordnen: 1) Solche, bei denen wegen häufiger Vexirfehler gar keine sichere Schwelle festzustellen war, 2) Solche, die neben häufigen Vexirfehlern doch eine Schwelle mehr oder weniger deutlich zeigten, und 3) Solche, die im Anfange Vexirfehler machten, aber nachher dazu gelangten eine deutliche Schwelle zu zeigen.

Zur ersten Classe gehörte Herr Dr. F. Kiesow. Es ist uns mit aller Mühe nicht gelungen, eine Schwelle auf der Volarseite des rechten Vorderarms festzustellen, welche für eine halbe Stunde annähernd constant blieb. Die Versuche wurden aufsteigend und absteigend angestellt. Am 25. Februar 1895 schien z. B. die Schwelle

1) Judd, Die Raumwahrnehmung im Gebiete des Tastsinnes, Philos. Stud. Bd. XII. S. 452, 455.

23 mm; am 28. Februar 14 mm; am 1. März konnte überhaupt keine Schwelle gefunden werden, die Antwort lautete immer »zwei Spitzen«; am 4. März schien die Schwelle wieder bei 23,5 mm zu sein; am 7. März war sie bei 5 mm. Da es möglich war, dass Nachempfindungen irgend einen Einfluss haben konnten, wurde die Zwischenzeit der einzelnen Versuche verändert (von 1 bis 30 Secunden), während die Spitzen in der Regel auf der Hand blieben, bis die Antwort abgegeben wurde. Bei 1 Secunde Zwischenzeit z. B. und 20 Versuchen bei einer constanten Entfernung der Spitzen von 23 mm nahm K. die Reize 6 Mal als 2 und 14 Mal als 1 wahr. Bei 2 Secunden Zwischenzeit erschienen die Spitzen immer als eine. Bei 3 Secunden und 20 Versuchen wurden sie immer als zwei wahrgenommen. Bei 4 Secunden gleichfalls als zwei. Bei 5 Secunden und 20 Versuchen 3 Mal als eine, u. s. w.

Zur ersten Classe der Versuchspersonen gehörte ferner Herr Dr. Brahn. Auch gelang es hier nicht, trotz der längeren Fortsetzung der Versuche, den widersprechenden Charakter der Antworten zu ändern. Man kann diese eigenthümliche Erscheinung, dass keine sichere Schwelle aufzufinden möglich war, keineswegs durch Schwankungen der Aufmerksamkeit oder sonstige ungünstige Bedingungen von Seiten der Versuchspersonen erklären, denn diese waren sonst sehr gute Beobachter. Diese Versuche ganz einfach als unbrauchbares Versuchsmaterial zu verwerfen, würde daher nicht richtig sein. Die Bedingungen der Versuche waren übrigens noch im allgemeinen dieselben wie bei den Herren Hef., A. und P. und wie bei den Volkmann'schen und Dresslar'schen Versuchen.

Derselben Classe gehörte schließlich auch Herr Stratton an, bei dem, wie auch bei Dr. K. und Dr. B., von Anfang an die Erscheinung der »Vexirfehler« vorhanden war.

Der zweiten Classe ist allein Herr Chamdanjian zuzuzählen. Die Hautstelle, die zur Einübung ausgewählt wurde, war am Hals. Das Eigenthümliche in diesem Falle war, dass die Versuchsperson sehr oft lauter Vexirfehler zu machen anfang; wurde ihr jedoch davon Mittheilung gemacht, so konnte sie das Urtheil selbst verbessern und eine deutliche und trotz Variationen in der Zahl der Abstufungen eine constante Schwelle zeigen. Manchmal machte sie am Anfang keine Vexirfehler, manchmal jedoch trotz des besten Willens nichts

als solche. Es war also ein sonderbar wechselndes Verfahren, das wir zunächst gar nicht verstehen konnten. Leider mussten auch hier aus äußeren Gründen die Versuche abgebrochen werden. Die Schwellenwerthe zeigten übrigens trotz längerer Versuche keine Verkleinerung.

Von der dritten Classe, bei der am Anfange sehr viele und später vergleichsweise sehr wenige Vexirfehler gemacht wurden, sei zunächst Herr Ladd erwähnt. Die Schwellen wurden auf 18 verschiedenen Hautstellen bei dem auf- und absteigenden Verfahren zu bestimmen versucht. Nach vieler Mühe wurden schließlich 18 annehmbare Schwellenwerthe erhalten. Sie waren aber auffallend klein und stimmten mit den Schwellenwerthen bei anderen Versuchspersonen an keiner der Stellen überein. Infolge dessen wurde nur das aufsteigende Verfahren angewandt und der Versuchsperson von den Ergebnissen Mittheilung gemacht. Sie wurde auch aufgefordert, die Antwort »zwei Spitzen« bei allen weiteren Versuchen nur abzugeben, wenn sie vollständig überzeugt sei. Dasselbe war auch schon den vorhin genannten Versuchspersonen gesagt worden, aber ohne denselben Erfolg. Es wurde nun 19 Tage hindurch die Schwelle auf der Volarseite des linken Vorderarms täglich bestimmt, und die folgenden Werthe erhalten: 60, 52, 60, 64, 54, 52, 55, 50, 42, 40, 45, 40, 45, 40, 38, 43, 38, 41, 42 mm. Im Vergleich zu den Resultaten von Volkmann und Dresslar, wie auch zu den früheren von uns selbst, war hier fast gar keine Einübung zu erkennen. Nach dieser Reihe wurden die Schwellen für die 18 Hautstellen noch einmal bestimmt. Sie waren in der Regel 2 bis 4mal größer als die vor der Einübungsreihe bestimmten Schwellenwerthe.

Ferner gehörte zur dritten Classe Herr H. Eber. Die Versuche an ihm hatten den Zweck: 1) alle Vexirfehler zu vermeiden, und 2) zu constatiren, ob es eine eigentliche Verkleinerung der Schwelle durch Einübung gebe. Die Versuchsperson wusste im Anfange nichts von Vexirfehlern überhaupt, auch nichts von einer Verkleinerung der Schwelle durch Einübung, außer dem, was sie durch den Titel der Arbeit vermuthen konnte. Es wurde nachher zufällig gefunden, dass sie doch am Anfange eine Verkleinerung der Schwelle erwartete. Zunächst wurden die Schwellenbestimmungen auf sechs verschiedenen Hautstellen gemacht, sodann eine Einübungsreihe für 13 Tage auf

der Dorsalseite des linken Handgelenks. Dann wurden die sechs Schwellenwerthe wieder bestimmt. Sehr oft antwortete Herr E. »zwei Spitzen«, wenn die Entfernung der Spitzen weit unter der Schwelle war: sein Verfahren zeigte im allgemeinen dieselben Resultate wie das der Herren Hef., A. und P. Nach der zweiten Bestimmung der sechs verschiedenen Schwellenwerthe wurde die Einübungsreihe fortgesetzt, und nach fernerem 13 Tagen wurden die sechs Schwellenwerthe zum dritten Male bestimmt. Tabelle VIII zeigt die erste Einübungsreihe.

Tabelle VIII.

Beob.: E. Linkes Handgelenk.

Datum	XI. 8.	11.	12.	13.	14.	15.	16.	18.	19.	21.	22.	23.	25.
linkes Handgelenk	18	16	10	12	13	12	14	13	13	9	10	9	6

Tabelle IX zeigt die Bestimmungen auf sechs Hautstellen, die vor und nach der ersten Uebungsreihe vorgenommen wurden.

Tabelle IX.

Beob.: E. 6 Hautstellen vor und nach der ersten Einübung.

Datum	Eingeübtes l. Handgelenk	Symmetrisches r. Handgelenk	linker Vorderarm	rechter Vorderarm	linker Oberarm	rechter Oberarm
XI. 2.—9.	18	28	22	18	55	30
XI. 25.—30.	6	8	7	12	53	12
Relativer Werth der Einübung	$\frac{18:6}{3}$	$\frac{28:8}{3\frac{1}{2}}$	$\frac{22:7}{3\frac{1}{7}}$	$\frac{18:12}{1\frac{1}{2}}$	$\frac{55:53}{1\frac{2}{53}}$	$\frac{30:12}{2\frac{1}{2}}$

Aus dieser Tabelle ist nichts Neues zu ersehen: sie zeigt dieselben Verkleinerungen, und es waren auch dieselben Vexirfehler vorhanden wie bei Herrn P. Um nun zu sehen, ob die Versuchsperson nicht andere Antworten zu geben im Stande sei, wurde ihr alles über die Vexirfehler, die Art ihres Zustandekommens mitgetheilt, ja sogar die eigenen Resultate und das ganze Verfahren. Darauf folgte die zweite Einübungsreihe, die in Tabelle X dargestellt ist.

Tabelle X.

Datum	XII. 2.	3.	4.	5.	6.	7.	9.	10.	11.	12.	13.	14.	16.
linkes Handgelenk	12	17	15	22	19	19	17	22	23	20	18	14	14

Hierauf wurde betont, dass es für diese Zwecke gar nicht darauf ankomme, dass die Schwellenwerthe sich klein zeigten; die Versuchsperson solle sich niemals bemühen, zwei Reize zu unterscheiden, sondern sich ganz receptiv verhalten. Es wurde auch sorgfältig versucht, alle Anstrengung nach einer Unterscheidung der zwei Reize zu vermeiden, die Versuchsperson sollte in der Versuchsstunde die Aufmerksamkeit auf gar nichts als die gegenwärtigen Eindrücke richten. Tabelle XI zeigt die auf die zweite Einübungsreihe folgenden allgemeinen Bestimmungen.

Tabelle XI.

Datum	Eingeübtes l. Handgelenk	Symmetrisches r. Handgelenk	linker Vorderarm	rechter Vorderarm	linker Oberarm	rechter Oberarm
XI. 25.—30.	6	8	7	12	53	12
XII. 16.—20.	12	16	12	11	20	18
Relativer Werth der Einübung	$\frac{6:12}{\frac{1}{2}}$	$\frac{8:16}{\frac{1}{2}}$	$\frac{7:12}{\frac{7}{12}}$	$\frac{12:11}{\frac{1}{11}}$	$\frac{53:20}{\frac{2^{13}}{20}}$	$\frac{12:18}{\frac{2}{3}}$

Es ist aus Tabelle XI ersichtlich, dass die Schwelle der eingeübten Stelle am Ende 12 mm anstatt 6 mm, wie am Ende der früheren Reihe (Tab. IX), war. In der Zwischenzeit der zwei Reihen wurden nur die allgemeinen Bestimmungen gemacht; und wenn man eine allgemeine Verkleinerung der Schwelle als das Resultat aller Einübung annimmt, so müsste die Schwelle der eingeübten Stelle sich während der Zeit der sechs Schwellenbestimmungen eigentlich immer noch verkleinert haben. Die Vergrößerung der Schwelle ist daher nicht durch eine Herabsetzung der Empfindlichkeit der Haut, sondern durch neue Bedingungen des Urtheils zu erklären. Während der zweiten Einübungsreihe wurde diese Schwelle zunächst grösser:

von 12 mm im Anfange erreichte sie ein Maximum von 23 mm. Dann ging sie für einige Tage schnell zurück, wahrscheinlich weil die alten Urtheilsbedingungen sich unwillkürlich wieder herstellten. Um die Verschiedenheiten in kurzer Uebersicht darzustellen, seien die folgenden Curven gegeben (Fig. 4 und 5). Schon aus diesen Re-

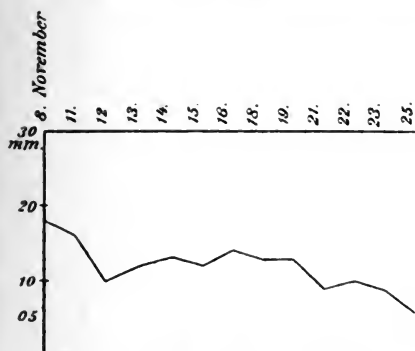


Fig. 4.

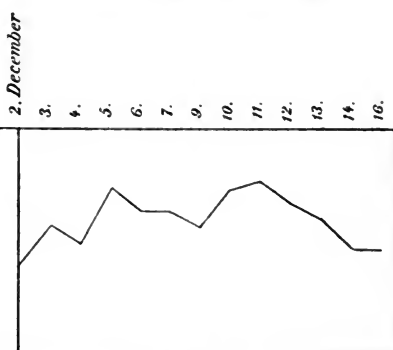


Fig. 5.

sultaten wird ersichtlich, dass die allerwichtigsten Momente dieser ganzen Einübungsfrage in den subjectiven Bedingungen der Wahrnehmung liegen. Sobald die Versuchsperson sich darüber klar wird, dass es gar nicht darauf ankommt, eine möglichst kleine Schwelle zu zeigen, dass es dagegen sehr darauf ankommt, dass die Antwort dem Reizobject angemessen sei, so erscheint keine Einübung mehr und auch kein Vexirfehler. Hierbei erhebt sich aber vor allem die Frage, welches die Bedingungen sind, die diese Veränderung in der Antwort verursacht haben. Dies schließt zugleich die Frage nach der Entstehung des Vexirfehlers ein. Dabei bleibt immerhin möglich, dass auch eine wahre Einübung in der Wahrnehmung zweier Punkte, die sich hauptsächlich in einer Verkleinerung der Schwelle zeigt, stattfinden kann.

Aus den vorliegenden Erfahrungen ergibt sich, dass das Vorkommen des Vexirfehlers weit häufiger bei einigen Versuchspersonen als bei anderen ist. Außerdem sieht man bei einigen Versuchspersonen, die Neigung, eine beträchtliche Pause zwischen dem Reiz und dem Urtheil einzuschieben, wodurch das Urtheil langsam vollzogen wird. Man kann wohl annehmen, dass die Versuchsperson in Fällen, wo diese Pause vorkommt, über die Beschaffenheit der Empfindung

reflectirt. Um dies zu vermeiden, wurde sie in einigen Fällen gebeten, das Urtheil sofort nach dem Empfinden des Aufsetzens abzugeben; doch es erschien kein Unterschied in den Resultaten. Bei den Herren Ch. und Dr. K. wurde ferner versucht, das Reflectiren durch kürzer dauerndes Aufsetzen der Spitzen zu vermeiden. Beide Vorsichtsmaßregeln erwiesen sich schließlich als ungenügend, die Vexirfehler auszuschließen. Es ließ sich aber feststellen, dass Solche, welche dieser Verlangsamung des Urtheils am stärksten zugeneigt waren, im allgemeinen die nämlichen sind, welche Vexirfehler am häufigsten machen. Auch bei denjenigen Versuchspersonen, welche eine große Verkleinerung der Schwelle durch die fortgesetzte Wiederholung von Schwellenbestimmungen zeigten, erschien es sicher, dass die Verlangsamung des Urtheils bei der Fortsetzung der Versuche größer wurde. Bei Herrn Hef. z. B. verspätete sich das Urtheil, das im Anfange der Versuche immer sofort abgegeben wurde, nach einer Woche im allgemeinen sehr beträchtlich.

Auch in Bezug auf die schwankenden oder zweifelhaften Urtheile, die mit größerer oder geringerer Häufigkeit bei jeder Versuchsperson erscheinen, zeigt es sich im allgemeinen sehr deutlich, dass dieselben entschieden häufiger sind bei denjenigen Versuchspersonen, die am häufigsten Vexirfehler machen, als bei den anderen. Auch bei den Herren Hef., A. und P. waren solche Urtheile am Ende häufiger als am Anfange der Versuche. Man kann daher annehmen, dass sie dadurch entstehen, dass die Versuchsperson aufmerksamer wird und ihre Urtheile immer genauer zu machen sucht. Es sei aber noch weiter bemerkt, dass am Ende der Versuche bei den Herren Hef., A. und P. die zweifelhaften Fälle, die im Anfange einfach »unentschieden« blieben, wie auch alle solchen Fälle bei den Herren Dr. B. und Dr. K., in der Regel schließlich als »wahrscheinlich zwei« angegeben wurden. In solchen Fällen entsteht die Verlangsamung des Urtheils offenbar aus dem Zweifel, ob die nähere Beschaffenheit der Empfindung aus einer doppelten Berührung der Haut entstanden sei. Nun mag man annehmen, dass durch eingehendere und aufmerksamere Beobachtung der Empfindung oder durch vergrößerte Empfindlichkeit der Haut selbst feinere Qualitätsunterschiede in der Empfindung bemerkt werden. Wie aber kann die Versuchsperson durch dieses Verfahren dazu kommen, zwei Punkte zu empfinden,

wo nur einer ist? Hier kann man vielleicht annehmen, in solchen Fällen sei die Versuchsperson nicht genügend aufmerksam, oder sie reflectire. Dieser Annahme widerspricht aber die Thatsache, dass durch eine schärfere und längere Beobachtung der Empfindung die zwei scheinbaren Punkte immer noch deutlicher bewusst und vollständiger von einander getrennt werden. Auch wenn die Versuchsperson die eine Spitze, welche auf der Haut steht, mit den Augen geradezu ansieht, empfindet sie sehr oft doch noch zwei Reize. Solche Fälle zeigen unverkennbar, dass es sich hier um etwas anderes als um eine erhöhte Aufmerksamkeit oder Empfindlichkeit handelt. Vielleicht kann man annehmen, dass Irradiations- oder Nebenerscheinungen der Empfindung bei einigen Versuchspersonen so stark hervortreten, dass sie dieselben von thatsächlichen Reizen nicht unterscheiden können. Offenbar bedarf aber die Punktförmigkeit und die Begrenzung der empfundenen Punkte auf nur zwei Punkte einer Erklärung durch anderweitige subjective Bedingungen.

In Bezug auf die Sicherheit des Antwortens wurden die folgenden Thatsachen beobachtet. Die Personen, die Vexirfehler begehen, insbesondere wenn sie schon wissen, dass sie manchmal solche Fehler gemacht haben, haben sehr oft das Gefühl, nachdem sie das Urtheil abgegeben, dass es unmöglich sei, unter diesen Versuchsbedingungen sichere Urtheile zu bilden. Diese Zweifel traten besonders stark in den Versuchen des Herrn Dr. B. und bei mir hervor. Die Versuchsperson will in der Regel durchaus nicht glauben, dass sie sich täuschen lässt, aber es bleibt ein Gefühl der Unsicherheit zurück. Als Versuchsperson in den Vorversuchen bemerkte ich, dass bei der Fortsetzung der Versuchsreihe diese Unsicherheit immer größer wurde. Nicht nur wurde das Urtheilen verlangsamt und schwankend, sondern schließlich hatte ich das Gefühl, dass möglicherweise ein anderes Urtheil unter anderen subjectiven Bedingungen, die ich doch nicht genauer erkennen konnte, ebenso richtig erschienen wäre, wie das abgegebene. Wegen der Einheit des Bewusstseins und jedes Apperceptionsactes sind wir unfähig uns ein Object vorzustellen, das z. B. dem Gesichtssinn als eins und zugleich dem Tastsinn als zwei erscheint. Wo wir ein Object mit allen Sinnen wahrnehmen, prüfen wir im gewöhnlichen Leben die Eindrücke eines vergleichungsweise sehr wenig entwickelten Sinnes an den Eindrücken des höher ent-

wickelten. Wenn z. B. die Versuchsperson einen Vexirfehler begeht und sich nachher die eine Spitze, die auf der Haut steht, ansieht, so glaubt sie nicht mehr, dass zwei Spitzen da sind, obgleich sie die zwei Tasteindrücke immer noch deutlich empfindet. Daher entsteht der große Einfluss der sogenannten Gesichtsassociationen auf die Wahrnehmung räumlicher Gestalten durch den Tastsinn, den Miss Washburn¹⁾ und Pillsbury²⁾ hervorgehoben haben. Manche Versuchspersonen erklären, dass im Moment des Aufsetzens der Spitzen keine Gesichtsvorstellungen in der Regel vorhanden seien. Dies wurde besonders von den Herren Dr. B. und Dr. Mentz angegeben; andere wie Hef., P., A., Mosch gaben an, dass Gesichtsvorstellungen immer zu beobachten seien. Nun zeigt sich, dass die Beobachter, welche die stärksten Gesichtsvorstellungen angeben, gerade diejenigen sind, welche im Anfange ihrer Versuche die größten und constantesten Schwellen haben und die wenigsten Vexirfehler begehen. Von Herrn Dr. M. wird angegeben, dass auch er, um das Urtheil zu vollenden, ein »geometrisches Bild«, d. h. eine Gesichtsvorstellung als Hilfsmittel hervorrufe. Die Thatsachen, welche bei der Angabe des Herrn Dr. M. zum Ausdruck kommen, scheinen demnach diese zu sein: 1) sein Urtheil beruht in erster Linie auf einer genauen Beobachtung der beim Aufsetzen der Spitzen erzeugten Empfindung; 2) für die Beurtheilung dieser Empfindung machen sich gewisse Gesichtsvorstellungen geltend.

Hier ist eine vierte allgemeine Beobachtung wichtig: Nicht nur die Zeitdauer, die Schwankungen und die relative Sicherheit des Urtheils zeigen sich bei verschiedenen Versuchspersonen und bei derselben Versuchsperson in verschiedenen Stadien ihres Verfahrens verschieden, sondern auch die Richtung der Aufmerksamkeit bietet beim Vollziehen des Urtheils beträchtliche Verschiedenheiten dar. Am Anfange der Versuche bei den Herren Hef., A., P. und mir selbst ist die Aufmerksamkeit ganz objectiv gerichtet. Der Empfindungen als solcher waren sich diese Versuchspersonen während der Versuche im Anfange überhaupt nicht bewusst. Die Frage, die sie an sich stellten, war rein objectiv: wie viele Spitzen berühren die Haut, eine

1) Philos. Stud. XI. S. 190 ff.

2) Amer. Journ. of Psych. VII. S. 42 ff.

oder zwei? Bei der Fortsetzung der Versuche wird man sich aber der Empfindungen immer deutlicher bewusst, bis schließlich vom Reizobject überhaupt abstrahirt wird und die Beschaffenheit der beim Aufsetzen der Spitzen erzeugten Empfindungen allein in Betracht kommt. Bei mir war diese Uebertragung der Aufmerksamkeit vom Reizobject auf die Empfindung so deutlich, dass ich zuerst geneigt war anzunehmen, dass die ganze sogenannte »Einübung« eigentlich keine andere Bedeutung habe, als dass Qualitätsunterschiede der Empfindung auf diese Weise zur Geltung kämen, welche in der gewöhnlichen Erfahrung gar nicht in Betracht kommen. Hiermit stimmen die Angaben der Herren Dr. B. und E. überein, dass der erste Gegenstand der Aufmerksamkeit bei ihnen die erregte Empfindung sei. »Die Aufmerksamkeit«, gibt Herr Dr. B. an, »wird momentan nach dem betreffenden Punkte gerichtet: immer kommt dann zunächst ein diffuser Reiz, der sich hierauf zu theilen scheint.« Mit dieser Angabe stimmt die erste von Herrn E., die am Anfange der ersten Versuchsreihe gemacht wurde, fast durchaus überein. Nur gab er an, dass ihm der objective Reiz sofort als doppelt erscheine. Da dies der einzige Unterschied zwischen den Aussagen dieser Versuchspersonen ist, so erhebt sich die Frage, ob die Unterschiede in den Resultaten beider damit irgendwie in Verbindung stehen. Herr Dr. B. selbst gab auch später an, sehr oft komme noch ein Punkt auf einer in der Nähe liegenden Stelle der Haut zum Vorschein, der mit jenem ersten verbunden oder von ihm ganz getrennt sein könne. »Gewöhnlich«, sagt ferner Herr E., »bleibt das Urtheil schwankend, und ich warte einen Augenblick, um zu erfahren, ob der Reiz nach längerer Wirkung sich nicht als zwei zeigen will.« Dass die Empfindung unter Abstraction vom Reizobjecte in diesem Falle, wie auch bei den Herren Dr. M. und Strat., welche ähnliche Angaben gemacht haben, als das einzige Datum, worauf das Urtheil beruht, angenommen wird, ist kaum zu bezweifeln.

Im Gegensatz zu dieser Thatsache steht nun das Verfahren der Versuchspersonen in denjenigen Fällen, bei denen keine Einübung eingetreten ist, und wo keine Vexirfehler begangen werden. Von Herrn E. wurde gegen das Ende der zweiten schon beschriebenen Versuchsreihe noch eine Angabe über sein Verfahren gemacht. Es sei hier hervorgehoben, dass er eine frühere ähnliche Angabe schon

ganz vergessen hatte. »Wenn es eins oder zwei ist, sage ich es sofort, und wenn ich im Zweifel bin, antwortete ich unentschieden.« Er wurde weiter gefragt, worauf sein Urtheil beruhe, und er schien überrascht, dass an etwas anderes als die Berührung der Zirkelspitzen gedacht wurde. Von Empfindungen wurde nichts gesagt, und er zögerte keinen Augenblick zwischen dem Reiz und dem Urtheil. Ebenfalls von Herrn Hef. wurde am Anfange seiner Versuche angegeben, dass er, während die Augen geschlossen waren, eine Vorstellung von dem Arm, dem Tisch worauf der Arm liegt (d. h. von einem kleinen Theil desselben, der in der unmittelbaren Nähe des Armes ist), dem Aufsetzen des Zirkels u. s. w. in dunklen Umrissen immer noch behalte. Er war ebenfalls überrascht, dass an etwas anderes als das Aufsetzen der Spitzen als Object der Aufmerksamkeit gedacht werden konnte. Bei Herrn M., dessen Schwelle durchaus constant und von Vexirfehlern frei blieb, war dies ebenfalls der Fall; nur fügte er noch hinzu, dass das Moment des Aufsetzens der Spitzen für ihn das allerwichtigste sei. In diesem Augenblick, gibt er an, wird das Urtheil bestimmt. Auch in den späteren Versuchen bei Herrn Ladd wurde bemerkt, dass die zwei Momente, das des Aufsetzens und das des Wegnehmens, zusammenfielen. Weiteres konnte aber nicht gefragt werden, ohne die Antwort zu suggeriren. An dieser Stelle bemerken wir, dass es sich in solchen Fällen um ein rein objectives Urtheil handelt, für welches eine längere analysirende Betrachtung der Qualitätsunterschiede der Empfindung selbst in keiner Weise maßgebend ist. Es geht sehr deutlich hervor, dass diese Versuchspersonen nie »zwei Spitzen« antworteten, bis sie überzeugt waren, dass die Tastempfindung mit der Vorstellung von zwei objectiven Spitzen zusammenpasste. Man darf hier annehmen, und meine eigene Selbstbeobachtung bestätigt es, dass bei allen diesen Versuchspersonen die aus der Erfahrung hervorgehende Verbindung zwischen der Vorstellung des objectiven Reizes und dem Tasteindruck für das Urtheil schließlich maßgebend ist. Die Antwort ist in solchen Fällen eine Deutung nach früherer im gewöhnlichen Leben gemachter Erfahrung, wo man, ausgenommen wenn die Entfernung der Tasteindrücke sehr groß ist, fortwährend seine Tasteindrücke an den Gesichtseindrücken prüft. Wenn man darüber entscheiden will, ob die Gesichtsvorstellung eines Objectes illusorisch sei, so pflegt

bekanntlich der Tastsinn entscheidend zu sein. Gerade umgekehrt erscheint die Beziehung zwischen Gesichts- und Tastvorstellungen bei der Wahrnehmung räumlicher Unterschiede. Wo es unmöglich ist, wie bei unserem Versuchsverfahren, den Tasteindruck bei minimalen Distanzen an dem Gesichtseindruck zu prüfen, wird der Grad des Glaubens, dass, wenn die Versuchsperson die Spitzen sehen könnte, sie eine oder zwei sehen würde, für das Urtheil maßgebend. Zwischen der gesehenen Entfernung der Spitzen, z. B. 40 mm, bei welcher sie durch den Tastsinn als zwei empfunden werden, und den Tasteindrücken selbst besteht eine enge Beziehung: wenn eine Versuchsperson die scheinbare Entfernung der bei dem Vexirfehler wahrgenommenen Empfindungen angibt, so scheint diese Entfernung in der Regel nahe der thatsächlichen Schwelle für die gereizte Hautstelle zu liegen, wie schon von Nichols¹⁾ und von Henri und Tawney²⁾ bemerkt wurde. Diese Beobachtung stimmt mit der Annahme überein, dass das scheinbare Vorhandensein von zwei Tasteindrücken von minimaler Entfernung in der Regel an den Gesichtsvorstellungen geprüft wird.

Wir können unsere Versuchspersonen in zwei Classen theilen, je nachdem sie die bloße Tastempfindung als für das Urtheil maßgebend annehmen oder nicht. In die erste Classe fallen diejenigen Versuchspersonen, welche die Empfindung vom Reizobject unterscheiden: Str., Dr. K., Dr. Br., Dr. M., Cham., E., L., Ar. und ich selbst. Die Verwerthung von Empfindungen zu Urtheilen wurde bei E., wie seine Aussagen beweisen, im Laufe der Versuche beseitigt. Wahrscheinlich geschah eine ähnliche Veränderung bei L., dessen verschiedenartige Resultate wir schon besprochen haben. In die zweite Classe fallen diejenigen, welche sich mit psychologischen Analysen und Unterscheidungen weniger beschäftigt haben: Hef., P., M. und andere. Im Anfange der Versuche schienen diese Versuchspersonen niemals das Reizobject von den Empfindungen zu unterscheiden.

Diesen Thatsachen stellen wir die folgenden Erscheinungen gegenüber, die gleichfalls aus den vorliegenden Versuchen hervorgehen: Die Versuchspersonen der ersten Classe geben entschieden mehr

1) Nichols, *Our Notions of Time and Space*. Boston 1894. S. 161.

2) *Philos. Stud.* XI. S. 400.

Vexirfehler überhaupt an, als die der zweiten: bei manchen derselben ist keine Schwelle der Vexirfehler wegen bestimmbar. Die der zweiten Classe zeigen im Gegentheil, wenigstens am Anfange ihrer Versuche, fast gar keine Vexirfehler und ferner eine sehr deutliche und von Versuch zu Versuch constante Schwelle. Durch die sogenannte Einübung aber, die sich uns schon als ein sehr zweifelhafter Vorgang erwiesen hat, werden die Personen der zweiten Classe allmählich zu Mitgliedern der ersten. Das Urtheil wird langsamer, schwankender, unsicherer: die Aufmerksamkeit wird auf die Tasteindrücke allein gerichtet; Vexirfehler werden schließlich häufig und die Schwelle außerordentlich klein, wenn nicht ganz verschwindend.

Aus den beschriebenen Versuchen geht deutlich hervor, dass die scheinbare Verkleinerung der Schwelle und der in enger Beziehung damit stehende Vexirfehler zwei überall auf der Hautoberfläche erscheinende Producte der sogenannten Einübung sind. Daraus ist zu schließen, dass die scheinbare Verkleinerung und der Vexirfehler an centrale, und zwar an psychische Vorgänge gebunden sind. Es entsteht daher die Frage: welches sind diese die Schwellenverkleinerung und den Vexirfehler bedingenden psychischen Vorgänge? Betrachten wir die allgemeinen Bedingungen der Versuchsanordnung, so ist ersichtlich, dass alle diese Bedingungen, außer einer einzigen, einer Variation unterworfen waren. Diese war das Vorherwissen des Zwecks und der Art der Versuche selbst. Jede Versuchsperson wusste über die Versuche selbst am Anfange alles, was der Experimentator selbst wusste. Der nächste Schritt war offenbar, in weiteren Versuchen auch diesen Factor möglichst zu variiren. Vor allem sollte die Aufmerksamkeit der Versuchspersonen womöglich gleichmäßig auf das Reizobject gerichtet, und außerdem dafür gesorgt werden, dass dieselben von dem Zweck und der Methode der Versuche absolut nichts wussten.

III.

Zunächst wurde die Titelangabe der Untersuchung verändert (sie wurde nur allgemein als »Untersuchung über Erkennung räumlicher Distanzen« bezeichnet), damit die Versuchspersonen nicht von vornherein durch den Titel suggestiv beeinflusst werden konnten. Die Versuchspersonen wurden so gewählt, dass sie keine oder nur unbestimmte

Kenntnisse von bisherigen Untersuchungen auf diesem Gebiete be-
 saßen. Sie sollten zunächst bloß angeben, was die Haut berührte.
 Zwischen der Versuchsperson und dem Experimentator wurde ein
 schwarzer Pappschirm aufgestellt und durch eine Oeffnung desselben
 der Arm gesteckt. Im übrigen blieben die allgemeinen Bedingungen
 der Versuche die nämlichen wie früher. Die Dauer der Einübungs-
 reihen war den Tagen nach gerechnet kürzer, aber die Zahl der
 einzelnen Bestimmungen an jedem Tage entschieden größer. Die
 allgemeinen Schwellenbestimmungen wurden weggelassen und die Unter-
 suchungen auf eine einzelne oder höchstens zwei Stellen beschränkt.
 Beobachter waren die Herren Franz, Mosch und Weyer. Die
 erste Reihe wurde an Herrn F. gewonnen. Tab. XII enthält die
 arithmetischen Mittel der Schwellenwerthe aus 9—12 Einzelbestim-
 mungen an jedem Tage.

Tabelle XII.

Datum	V. 18.	20.	21.	22.	23.	24.
Volarseite des Vorder- armes	26	24	27			14
Dorsalseite desselben	39	37	36	40	38	10

Nach 40 Einzelbestimmungen innerhalb fünf Tagen war keine
 Verkleinerung der Schwelle mehr ersichtlich. Am 24. Mai wurde,
 um den Einfluss der Einübung zu untersuchen, der Versuchsperson
 suggerirt, dass zunächst mehrere Schwellenbestimmungen innerhalb
 einer Stunde gemacht werden sollten, und es wurden ihr außerdem
 die Resultate der bisherigen Versuche mitgetheilt. Sofort wurden
 die Schwellenwerthe kleiner. Am vorangegangenen Tag (23. Mai)
 waren sie an der Dorsalseite des Vorderarmes gewesen: 40, 35, 35,
 35, 30, 45, 35, 35, 40, 40, 45 mm. Am 24. Mai waren sie: 14, 8,
 12, 14, 8, 6, 8, 12, 8, 2 mm.

Die zweite Versuchsreihe wurde bei Herrn Weyer ausgeführt.
 Stellen wir die arithmetischen Mittel aus 7—10 Einzelbestimmungen

zusammen, wie in der vorhergehenden Tabelle, so erhalten wir die folgenden Zahlen.

Tabelle XIII.

Datum	V. 4.	6.	11.	VI. 26.	27.	VII. 1.	2.
Volarseite des Armes	22	20	29	23	38	30	12

Zwischen dem 11. Mai und dem 26. Juni war eine längere Pause: wir können daher die gesammte Reihe eigentlich nicht als eine Einübungsreihe betrachten. Es wurde aber von Volkmann gefunden, dass an demselben Tage durch 13 einzelne Bestimmungen der Schwelle der Werth derselben von 0,75''' am Anfang auf 0,45''' am Ende sank. In den Versuchen bei Herrn W. wurden hier im Gegentheil z. B. am 1. Juli folgende Zahlen gewonnen: 30, 30, 30, 35, 35, 25, 25, 30, 30, 25 mm: in diesen Versuchen ist gar keine Verkleinerung ersichtlich. Am 2. Juli wurde Herrn W., wie früher Herrn F., suggerirt, dass der Einfluss der Einübung untersucht werden sollte. Darauf folgten sieben Bestimmungen, bei welchen eine Schwelle nur zwei Mal bestimmbar wurde, einmal bei 10 mm und einmal bei 14 mm. Infolge der Suggestion wurden also die Vexirfehler häufiger, und die Schwelle sank auf ein Drittel ihrer früheren Größe. In drei anderen Bestimmungen waren Vexirfehler zu häufig, um eine Schwelle zu erkennen, und in den zwei übrigen schienen wegen Ermüdung der Versuchsperson die Bedingungen für die Wirkung der Suggestion nicht mehr vorhanden zu sein: diese Schwellen waren deutlich 40 und 35 mm.

Die dritte und letzte dieser Versuchspersonen war Herr Mosch. In keiner Versuchsreihe waren Vexirfehler so wenig vorhanden wie bei ihm. Die Suggestionsversuche wurden am 18. und 19. Juni an- gestellt. Es wurde suggerirt, dass nun der Einfluss der Einübung untersucht werden sollte. Auf die Frage, warum die schon angeführten Versuche nicht als Einübungsversuche gelten dürften, wurde erwidert, dass die Uebungserfolge anderer Beobachter noch nicht erreicht seien. Um dies zu erzielen, sollte der Beobachter jetzt die Empfindungen sehr scharf fixiren und die Antworten möglichst exact

geben. Nachher wurde die Versuchsperson veranlasst, ihre alte Urtheilswaise wieder anzuwenden. In Folge dessen kehrte die Schwelle noch einmal zu der ursprünglichen Größe zurück. Die Mittel aus 6—12 Einzelversuchen sind:

Tabelle XIV.

Datum	VI. 2.	4.	9.	11.	13.	16.	18.	19.	25.	27.
Volarseite des Armes	31	36	28	43	44	48	30	9	37	43

Um die einzelnen Abweichungen der Schwellenwerthe genauer ersichtlich zu machen, sind in der folgenden Tabelle die einzelnen Bestimmungen angeführt.

Tabelle XV.

Datum	Schwellenbestimmungen.											
2. VI.	15	30	40	30	35	35	30	30				
4.	25	35	40	35	35	30	40	40	35	40		
9.	30	30	25	25	30	30						
11.	40	40	40	45	45	50	45	40				
13.	45	40	40	40	45	45	50	45	45			
16.	45	55	50	50	55	50	50	50				
18.	25	35	25	35	40	35	25	30				
19.	16	0	0	20	20	5	5	5	10	5		
25.	35	35	30	30	40	35	45	45				
27.	45	50	45	40	40	40	40	45	45	40	40	45

Diese Tabelle zeigt, dass die Schwelle im Anfang der Versuche kleiner war als am Ende, was dadurch zu erklären ist, dass die Versuchsperson anfangs ihre Aufmerksamkeit etwas schärfer auf die Versuche richtete als später. Anstatt einer Verkleinerung der Schwelle durch Einübung der Aufmerksamkeit geschah also in diesem Falle eine Zunahme derselben wahrscheinlich in Folge der Ablenkung der Aufmerksamkeit. Am 18. gelang die Suggestion nur sehr wenig: die Uebertragung der Aufmerksamkeit von dem Reizobject auf die vom Reizobject abstrahirten Empfindungen brauchte offenbar eine »Einübung«, die erst am 19. annähernd vollkommen war. Nebst der Verkleinerung der Schwelle an diesem Tage machte Herr M. die einzigen Vexirfehler, die er überhaupt angab. Auch hier nahm er eine Spitze, außer in zwei Fällen, in welchen das Urtheil »unsicher« war, immer richtig wahr, aber Entfernungen von 5 mm sehr oft als zwei Spitzen. Schließlich sagte er ganz spontan: »Uebrigens kann ich zwei Eindrücke immer empfinden, wenn ich will.« Nach diesen zwei Tagen wurde ihm gesagt, dass er nicht mehr versuchen solle, feinere Unterscheidungen zu machen, sondern ganz »grob«, wie vorher, die Zahl der Spitzen angeben. Am 25. wurden die Versuche wieder fortgesetzt, und die alte Schwelle war wieder vorhanden. Am 27. wurden die Bestimmungen in längerer zusammenhängender Reihe 12mal wiederholt mit Abstufungen der einzelnen Versuche von 5 mm. Die Schwellenwerthe waren außerordentlich klar und deutlich, nämlich entweder 40 oder 45 mm, wie aus der Tabelle ersichtlich ist. Die sehr bedeutende Schwellenverkleinerung, die bei Volkmann und Fechner innerhalb einer zusammenhängenden Reihe vorkam, ist hier gar nicht vorhanden. Auf- und absteigendes Verfahren konnten bei Herrn M. angewandt werden, ohne die Resultate zu ändern. Die Schwellenwerthe am 27. Juni wurden alle mittelst des aufsteigenden Verfahrens gewonnen.

Aus diesen Versuchen, besonders wenn sie mit den früheren bei den Herren Hef., A. und P. verglichen werden, geht sehr deutlich hervor, dass die sogenannte »Einübung« einerseits irgendwie an das Vorherwissen über den Zweck und die Methode der Versuche gebunden war, anderseits, dass durch das Ausschließen dieses Einflusses die »Einübung« abhanden kam. Nehmen wir nun dieses Resultat mit den schon besprochenen Thatsachen zusammen, dass

die Aufmerksamkeit durch »Einübung« allmählich auf die Empfindung anstatt auf das Reizobject gerichtet wird, und dass die Versuchsperson bei minimalen Distanzen glaubt, sich ganz und gar auf eine Analyse der Qualitätsunterschiede der Tastempfindung verlassen zu können, so sehen wir, dass diese Ergebnisse sich einigermaßen ergänzen. Die Versuchsperson, die den Zweck und die Methode der Versuche vorher weiß, verlässt den gewöhnlichen Weg des Prüfens ihrer Tasteindrücke an Gesichtsvorstellungen und glaubt sich fähig, auf der Basis ihres Vorherwissens und der Qualitätsunterschiede der Tastempfindung genauere Auskunft über den vorhandenen Reiz geben zu können. Wenn sie nicht wüsste, dass nur eine oder zwei Spitzen jedes Mal aufgesetzt werden, würde sie demnach auch noch mehrere Reize anscheinend empfinden können: in der That gab Herr St. an, saps er manchmal drei oder sogar fünf oder mehr verschiedene Reize deutlich empfinde, obwohl nur eine Spitze aufgesetzt wurde. In solchen Fällen abstrahirt wahrscheinlich die Versuchsperson nicht nur von allen Gesichtsvorstellungen, sondern auch von dem Vorherwissen selbst und richtet das Urtheil ganz nach den für sie scheinbar vorhandenen Qualitätsunterschieden der Tastempfindung. Einige von diesen Unterschieden sind anfängliche Schmerz- oder Temperaturempfindungen, die Irradiation des Reizes selbst, die diffuse Empfindung des Dehnens der Haut beim Aufsetzen der Spitze u. s. w. Dazu kommen auch die von vorangegangenen Versuchen nachgebliebenen Veränderungen in der Empfindlichkeit der Haut selbst. Das Vorherwissen über den Zweck und die Methode der Versuche kann deswegen keine zugängliche Basis für das Urtheil darbieten, weil es in jedem Falle mehrere Vorstellungen erlaubt. Die Vorstellung der zwei Punkte kann in jedem Fall als eine Suggestion wirken, welche zwei scheinbare Empfindungen hervorrufen kann. Von fast allen Versuchspersonen wird angegeben, dass nach dem Signal eine mehr oder weniger diffuse Empfindung auf der gebrauchten Hautstelle deutlich hervortrete, schon bevor der Reiz thatsächlich einwirkt. »Auf der Haut« — sagt in Uebereinstimmung hiermit H. Meyer — »gelingt es mir leicht, an welcher Stelle ich will, subjective Empfindungen (Wärme, Kälte, Druck) durch angespannte Vorstellung derselben hervorzu- bringen Die Empfindungen können so lebhaft werden, dass ich, ich mag wollen oder nicht, mit der Hand über die Hautstelle

hinstreichen muss, wie man es in Fällen örtlicher Hautempfindungen zu thun pflegt¹⁾. Berührt man eine angesehene Hautstelle mit einer Spitze und versucht es — nicht zu denken, dass zwei Spitzen da seien²⁾ — sondern zwei getrennte und punktförmige Tasteindrücke zu empfinden, so gelingt der Versuch der Mehrzahl von Personen. Man braucht bloß von der Gesichtsvorstellung der Spitze zuerst ganz zu abstrahiren, und der Versuch gelingt jedes Mal.

Wo man bei minimalen Distanzen sich ganz auf die durch frühere Erfahrung gewonnene Deutung des Tasteindruckes verlässt, ist die Fragestellung, welche bei der Antwort eintritt, ungefähr folgende: Ist die Tastempfindung, die ich jetzt erfahre, der Tastempfindung gleich, die ich immer gleichzeitig mit zwei Gesichtseindrücken erfahren habe? Ein sicheres Urtheil ist hierbei nur bei den größten Entfernungen der Spitzen möglich, z. B. 40 oder 45 mm auf dem Vorderarm, bei welchen die gewöhnlichen Schwellen liegen. Sieht nun die Versuchsperson von ihren Gesichtsvorstellungen und ihrer Erfahrung minimaler Distanzen ganz ab, so wird sie in dem Tasteindruck zunächst einen diffusen Reiz finden, welcher sich durch den Einfluss der Suggestion in mehrere Punkte zerlegen lässt.

Auf ähnliche Weise hat im Grunde schon Camerer den Vexirfehler erklärt. Er ist nach ihm ein Product der Einbildungskraft und der Erwartung³⁾. Auf Grund seiner ersten Versuchsreihe war er geneigt gewesen anzunehmen, dass die Erscheinung von äußeren Bedingungen abhängt, wie schiefes Aufsetzen der Spitzen, ungleichförmiges Aufdrücken derselben u. s. w. Er fand aber, dass der Vexirfehler nicht nur zu dem Urtheil »mehr als ein Punkt«, sondern auch zu dem deutlichen Eindruck von zwei Spitzen führte. Dies kann nun von solchen äußeren Bedingungen nicht wohl hervorgebracht werden. In neuerer Zeit ist eine ähnliche Ansicht auch von Nichols⁴⁾ vertreten worden. Fechner behauptet, dass der Vexirfehler, wo er innerhalb gemischter Versuchsreihen (d. h. wo bald eine, bald zwei Spitzen verwendet werden) vorkomme, eine Erscheinung des Contrastes

1) Fechner, Elemente der Psychophysik, Bd. II, S. 486, Leipzig 1889.

2) Dies scheint der nicht gelungene Versuch Fechner's gewesen zu sein. Fechner: Ueber die M. d. r. u. f. Fälle. Leipzig 1884. S. 137 u. f.

3) Zeitschrift f. Biologie, Bd. XIX, S. 280—300.

4) Nichols, Our Notion of Time and Space. Boston 1894. S. 156 ff.

sei¹⁾: wo er aber am Anfange einer solchen Versuchsreihe oder bei reinen Vexirversuchen vorkommt, schließt er sich der Theorie Camerer's an: »Die Entstehung der Vexirfehler an sich selbst aber scheint in der Hauptsache eine Sache der Einbildung zu sein, wenn schon die Mitwirkung anderer Momente dabei nicht ausgeschlossen ist.«²⁾.

IV.

Gehen wir nun zu einer kurzen Betrachtung einiger weiteren Erscheinungen der Vexirfehler über, wie sie in den Versuchen vorkamen, welche den Zweck hatten, das Vorkommen von Vexirfehlern zu beeinflussen. Dies geschah in zwei Richtungen. Zunächst wurde der Versuchsperson suggerirt, dass bloß eine Spitze in 10 Versuchen aufgesetzt werden würde, worauf das Resultat mit 10 »unwissentlichen« Vexirversuchen verglichen wurde. Sodann wurden der Versuchsperson zwei Spitzen gezeigt, aber bloß eine derselben aufgesetzt, und die Resultate abermals mit unwissentlichen Vexirversuchen verglichen. Nach der Ausführung von 14 Versuchsreihen bei Herrn Dr. M., die den Zweck hatten, die Schwelle zu bestimmen, war jedoch gar keine Schwelle ersichtlich. Da es unmöglich ist, die Art der Vexirversuche in Tabellen oder bloßen Beschreibungen wiederzugeben, sollen die folgenden Vexirversuche als Beispiele herausgegriffen werden. Zunächst wurde eine Spitze 10 Mal unwissentlich aufgesetzt und die folgenden Antworten gewonnen:

- 1) Zwei Spitzen gleichzeitig: die eine stärker und tiefer sinkend.
- 2) Eine linienartige Tastvorstellung.
- 3) Zwei — die eine stärker als die andere.
- 4) Zwei — eben merklich, durch eine linienartige Tastvorstellung verbunden.
- 5) Zwei — gleichstark, gleichzeitig und deutlich getrennt.
- 6) Zwei — eine Linie mit zwei Punkten an den Enden.
- 7) Zwei — dasselbe.
- 8) Eine Linie, die allmählich zwei Endpunkte zeigte.
- 9) Zwei — eben merklich verbunden.
- 10) Zwei — verbunden. Die Linie ist zunächst keine Gesichtsvorstellung.

Sodann wurde der Versuchsperson gesagt, dass nur eine Spitze aufgesetzt werde, sie solle aber die Empfindung unbefangen beschreiben. Darauf wurden die folgenden Antworten gewonnen:

1) Fechner, Ueber die Meth. d. r. u. f. Fälle. Leipzig 1884. S. 131 u. 137.
 2) a. a. O. S. 306.

- 1) Eine Spitze, aber ausgebreitet.
- 2) Eine Spitze.
- 3) Eine Spitze mit diffuser Ausbreitung.
- 4) Eine Spitze mit eben merklicher Ausbreitung.
- 5) Eine Spitze.
- 6) Dasselbe.
- 7) Eine Spitze, etwas ausgebreitet.
- 8) Dasselbe.
- 9) Dasselbe.
- 10) Eine Spitze.

Der berührte Hautpunkt war innerhalb $\frac{1}{8}$ mm der nämliche wie in den obigen 10 Versuchen. Der Punkt war mit Anilin markirt und wurde bei jedem Versuche möglichst genau getroffen. Die Versuche wurden an drei Tagen wiederholt, mit denselben Resultaten. Gleichzeitig wurden Versuche mit zwei Spitzen und mit Abstufungen von 5 mm angewandt, bis zur Entfernung von 20 mm: gleichwohl lautete die Antwort »eine Spitze«. Bei 20 mm bemerkte Herr Dr. M., »es könnten auch 2 sein: jedenfalls sei eine Ausbreitung da«. Wurde dann wieder eine Spitze aufgesetzt, so erschien dieselbe Antwort wie bei 20 mm: »es scheint, als ob zwei da waren«. Schließlich betrachtete die Versuchsperson die Hautstelle, während eine Spitze aufgesetzt wurde, fühlte jedoch zwei deutliche Empfindungen, eine an dem wirklichen Ort der Spitze oder auf der einen Seite derselben, die andere auf der anderen Seite, und zwar anscheinend in verschiedener Entfernung, zuweilen auch mit diffuser Verbindung. Bei Entfernungen von 5, 10, 15 und 20 mm, wo die Spitzen früher immer als zwei wahrgenommen worden waren, und wo der Reiz sehr diffus war, wurde doch in Folge der Suggestion, dass nur eine Spitze aufgesetzt werde, auch nur eine Spitze anscheinend wahrgenommen. Bei Herrn Str. wurde diese Erscheinung ebenfalls beobachtet und folgendermaßen geprüft: Ihm wurde eine Spitze gezeigt, während zwei bei einer Entfernung von 20 mm (d. i. etwas oberhalb der Distanz, die als die Schwelle der Stelle bestimmt war) aufgesetzt wurden. Die Antwort »zwei Punkte« wurde nur fünf Mal bei im ganzen 37 Versuchen gegeben, die andern 32 Wahrnehmungen erschienen als ein einzelner, mehr oder weniger ausgebreiteter, bald linienartiger, bald punktförmiger Reiz.

Gehen wir sodann zu den Beobachtungen über, bei denen versucht wurde, den Vexirfehler durch das Zeigen von zwei Spitzen zu

beeinflussen. Ein Theil dieser Versuche ist schon früher veröffentlicht¹⁾ worden. Deshalb sei hier bloß das Resultat wiedergegeben: »diese Tabellen zeigen deutlich, dass die Erwartung von zwei Punkten einen sehr bedeutenden Einfluss auf die Zahl und Art der Vexirfehler hat. Wir sehen nämlich, dass bei T. in den 49 Versuchen, in denen zwei Spitzen gezeigt wurden, zehn Mal ein Punkt und 39 Mal zwei Punkte wahrgenommen wurden: die andere Versuchsperson, Str., hat in 51 Versuchen 11 Mal einen Punkt und 40 Mal zwei Punkte angegeben; dagegen in den Versuchen, wo eine Spitze gezeigt und auch ein Punkt berührt wurde, hat T. 24 Mal in 28 Versuchen und Str. 14 Mal in 23 Versuchen einen Punkt empfunden«. Bei einer andern Versuchsperson hatten die gezeigten Spitzen einen in 5 mm abgestuften Abstand, den man zu Schwellenbestimmungen nach der Methode der Minimaländerungen hätte anwenden können. Dabei wurde die Versuchsperson veranlasst, nicht bloß die Zahl, sondern auch in Fällen, wo scheinbar zwei Spitzen wahrgenommen wurden, die wahrgenommene Empfindung anzugeben. Die Resultate sprechen sehr deutlich für den Einfluss der Suggestion, indem die angegebenen Entfernungen der angeblich wahrgenommenen Punkte eine ziemlich regelmäßig abgestufte Reihe bildeten. Dieselben Versuche wurden noch bei den Herren Fr. und M., die, wie schon angegeben, fast keine Vexirfehler machten, ausgeführt, aber ohne denselben Erfolg. Nach wenigen Versuchen, in denen die Spitze richtig wahrgenommen wurde, sagte die Versuchsperson ganz einfach: »Sie setzen die gezeigten Spitzen nicht auf die Haut«; sie ließ sich also gar nicht täuschen. Im allgemeinen ergibt sich aus diesen Versuchen das folgende Resultat: bei Versuchspersonen, die daran gewöhnt werden, Vexirfehler häufig zu begehen, wirkt die eben beschriebene Suggestion vollkommen; bei Versuchspersonen, die Vexirfehler überhaupt sehr selten machen, bleibt die Suggestion fast wirkungslos. Stellt man nun die Versuche über den Vexirvorgang, die schon ausgeführt worden sind, mit diesem Resultat zusammen, so kann man vielleicht annehmen, dass eine suggerirte Vorstellung schließlich bei allen minimalen Distanzen zum nächsten Maßstabe

1) Henri und Tawney, Ueber die Trugwahrnehmung zweier Punkte. Philos. Studien XI, S. 402 f., Tabellen V, VI.

der Wahrnehmung wird, dass man aber dieselbe in Folge von früheren Erfahrungen über die Tastempfindungen und in Folge eines hinzutretenden Abstractionsvorgangs auch wieder ablehnen kann.

Schon in den Versuchen von Herrn Cham. erschien das Vermeiden des Vexirfehlers in Folge solcher secundärer Vorgänge möglich. Er machte sehr oft im Anfange der Versuchsstunde so viele Vexirfehler, dass eine sichere Schwelle nicht bestimmt werden konnte, aber nachher wurde das Urtheil constanter. Auch bei anderen Versuchspersonen waren Vexirfehler an einigen Tagen häufiger als an anderen, obgleich die Hautstellen dieselben waren. Die Versuche bei den Herren Ladd und Eber seien auch hier wieder hervorgehoben. Im Anfange waren Vexirfehler bei ihnen sehr häufig; nachdem die Versuche mit jedem von ihnen ausführlich besprochen waren, wurden ihre Schwellenwerthe größer und constanter, und Vexirfehler erschienen fast gar nicht mehr. Ich habe selbst versucht, mit der Hülfe des Herrn H. A. Senter, die zwei Verfahrungsweisen willkürlich anzuwenden und Versuche an einem Tage nach der einen, an einem anderen Tage nach der andern auszuführen. Der Versuch gelang einigermaßen. Nach einiger Zeit war ich gar nicht mehr im Stande, die Trugwahrnehmungen von richtigen Wahrnehmungen zu unterscheiden. An anderen Tagen konnte ich jedoch die Spitzen in gemischten Versuchsreihen in der Regel richtig wahrnehmen. Sehr oft übte der Versuch, alle Suggestion zu vermeiden, gerade die entgegengesetzte Wirkung aus: dies ist, wie es scheint, in der Regel das Resultat, wenn eine Versuchsperson, die häufige Vexirfehler überhaupt begeht, Vexirversuche an sich selbst machen lässt. Um dies zu vermeiden, nahm ich mir vor jenen Suggestionsversuchen fest vor, die Tastempfindung allein bei jedem Versuch zu beobachten. Aber auch dies gelingt nicht leicht, wenn man nicht viele solche Versuche schon gemacht hat. Jedenfalls muss man von dem Reizobject und allen äußeren Umständen der Versuche ganz abstrahiren, und dies ist viel mehr eine Sache der Gewohnheit als der Willkür. Doch kann nicht geleugnet werden, dass unter Umständen auch die letztere einen Einfluss ausübt. In dieser Beziehung sind einige Beobachtungen an Herrn Dr. M. lehrreich. Nach mehreren Versuchen, bei denen Vexirfehler häufig auftraten, wurde der Unterschied der verschiedenen Versuchspersonen bezüglich des Vorkommens von Vexirfehlern mit ihm

besprochen, sodann die scheinbar genauere Analyse der Empfindungen, welche bei solchen Personen vorkommen, die Vexirfehler machen. Auch die Seltenheit der Vexirfehler bei manchen Beobachtern wurde erwähnt. Darauf wurden gemischte Reihen von Vexirversuchen an- gestellt, und zwar mit abwechselnd einer und zwei Spitzen: im letz- teren Falle betrugen die Spitzen immer dieselbe Entfernung, nämlich 30 mm. Das Resultat war, dass die Spitzen bei jedem Versuch richtig wahrgenommen wurden. Dies konnte vielleicht daher entstehen, dass der Contrast sehr stark war (Fechner). Darauf folgten aber 10 reine Vexirversuche, in denen die Spitze gleichfalls immer richtig wahr- genommen wurde — ein Resultat, das niemals früher beobachtet war, ausgenommen wenn es der Versuchsperson vorher suggerirt war, dass nur eine Spitze angewendet werde. Diese Reihen wurden nachher innerhalb derselben Stunde zweimal wiederholt, und mit nur vier Ausnahmen wurden die Spitzen richtig wahrgenommen. An einem späteren Tage wurden die folgenden vier Schwellenbestimmungen gemacht. Die linke Columne gibt den Abstand der Zirkelspitzen in Millimetern an, die rechte die Antwort der Versuchsperson. Die Hautstelle war auf der Dorsalseite des rechten Vorderarmes zwischen 9 und 12 cm von der Linie des Handgelenks nach dem Ellen- bogen hin.

1.		2.		3.		4.	
5 mm	1	5 mm	1	5 mm	1	5 mm	1
10	1	10	1	10	1	10—20	1
15—30	1	15—25	1	15—25	1	25	2
35	2	30	2	30	2	30	?
40	2	35	2	35	2	30	2
45	2	40	2	40	2	35	2

Man sieht aus diesen Bestimmungen eine deutliche und constante Schwelle, wo früher keine Schwelle bestimmt werden konnte. Auch zeigt sich noch einmal, dass das Begehen von Vexirfehlern von der Versuchsperson selbst vermieden werden kann. Die Frage, wie dies geschehen ist, ist aus den Thatsachen selbst nicht ohne weiteres zu beantworten; aber einige Aussagen des Herrn Dr. M. scheinen wohl zutreffend zu sein. Sein Verfahren erschien zunächst als ein Versuch die Wahrnehmungsweise anderer Versuchspersonen, deren sonstiges

Verhalten ihm bekannt war, sich vorzustellen und selbst nachzuahmen. Dabei bemerkte er folgendes: »ich passe sonst zu genau auf Nebenerscheinungen auf«, »man soll bloß fragen, was für ein Object die Haut berührt, soll die Empfindung von dem Object überhaupt nicht unterscheiden u. s. w.« Es schien dem Herrn Dr. M. günstig zu sein, die Aussagen möglichst rasch und die Zwischenzeit der einzelnen Versuche sehr kurz zu machen. Entscheidend ist offenbar, dass man versucht, bloß Aussagen über das äußere Object selbst zu machen, ohne sich durch die subjectiven Nebenerscheinungen der Empfindung beirren zu lassen; das zweite, die Verkürzung der Zwischenzeit, ist nur ein Hilfsmittel für das erstere. Von Camerer wurde bei seiner ersten Versuchsanordnung¹⁾ gefunden, dass, wo Vexirversuche zwischen Versuchen mit zwei Nadeln eingeschaltet sind, die Länge der Zwischenzeit einen beträchtlichen Einfluss ausübt. Dies Resultat wird auch durch die vorliegende Untersuchung bestätigt, aber die Thatsache lässt sich leicht übereinstimmend mit Fechner²⁾ durch den Contrast erklären, der in solchen Versuchen zur Geltung kommt. In seiner zweiten Versuchsanordnung³⁾ wählte Camerer sehr lange Zwischenzeiten, 5 Minuten und eine halbe Stunde, und der betreffende Einfluss fiel in Folge dessen hinweg. Bei reinen Vexirversuchen, wie auch bei Schwellenbestimmungen nach der Methode der Minimaländerungen mit kleinen Abstufungen, scheint aber der Contrast keinen beträchtlichen Einfluss auszuüben.

Vergleichen wir die Angaben des Herrn Dr. M. und die Erfahrungen bei Herrn E. mit den Angaben jener Versuchspersonen, bei denen umgekehrt eine große Verkleinerung der Schwelle stattgefunden hatte, so finden wir einen gerade entgegengesetzten Verlauf der Versuche, wie die folgende Uebersicht zeigt:

Die eingeübten Versuchspersonen	
Hef., Ar. und P.	Dr. M. und E.
Im Anfange: Aufmerksamkeit objectiv, Stellung gegen den Tasteindruck passiv, Hülfe für das Urtheil die mit dem Eindruck durch frühere Er-	Im Anfange: Aufmerksamkeit subjectiv auf die Tastempfindung gerichtet, Stellung gegen diese activ und analysirend, Hülfe für das Urtheil durch

1) Camerer, Zeitschrift für Biologie. Bd. XVII. S. 1 ff.

2) Fechner, Ueber die M. d. r. u. f. Fälle. Leipzig 1884. S. 131.

3) Camerer, a. a. O. Bd. XIX. S. 280 ff.

fahrung in Complicationsverbindung stehenden Gesichtsvorstellungen.

Am Ende: Aufmerksamkeit subjectiv auf die vom Reizobject abstrahirte Tastempfindung gerichtet, Stellung gegen diese Empfindung activ und analysirend, Hülfe für das Urtheil die Suggestion der Gesichtsvorstellungen, die auf die Empfindung wirken.

Gesichtsvorstellungen, die suggestiv auf die Tastempfindung wirken.

Am Ende: Aufmerksamkeit objectiv, Stellung gegen den Tasteindruck passiv, Hülfe für das Urtheil die mit dem Eindruck in Complicationsverbindung stehenden Gesichtsvorstellungen.

Die ganze sogenannte Einübung scheint hiernach eigentlich nichts anderes zu sein, als ein Process, wodurch ein Autosuggestionsverfahren im Bewusstsein der Versuchsperson sich abspielt: die Trugwahrnehmung zweier Punkte, der sogenannte Vexirfehler, erscheint in diesem Falle als ein Product der durch »Einübung« hergestellten Autosuggestion. Bei beiden Classen von Versuchspersonen aber scheint die Autosuggestion schließlich durch Abstraction von dem objectiven Eindruck und seinen Gesichtscorrelationen, sowie durch die Absicht, die Schwelle so klein und genau wie möglich zu machen, vorbereitet zu sein.

Noch bleiben uns einige weitere Eigenschaften des Vexirfehlers zu erörtern übrig. Nach dem Protocoll dieser Versuche scheinen die zwei Punkte des Vexirfehlers sehr oft weder gleichzeitig noch gleichstark noch gleichförmig zu sein. Die Versuchspersonen wurden absichtlich selten nach den Umständen des Auftretens der Vexirfehler gefragt, weil dadurch die Aufmerksamkeit zu sehr auf Nebenerscheinungen der Versuche gerichtet gewesen wäre. Soweit, wie diese Methode reichen kann, stellt sich heraus, dass die Thatsachen sich verschieden bei verschiedenen Versuchspersonen verhalten. Bei Herrn Stratton erscheinen die als angeblich zwei wahrgenommenen Punkte in der Regel gleichzeitig, gleichförmig und gleichstark. Bei Herrn Dr. M. und Dr. Br. war dies nicht der Fall. Es sei aber bemerkt, dass bei früheren Versuchen über den Vexirfehler, wo die Versuchsperson immer um eine ausführlichere Beschreibung gebeten wurde, die Punkte in der Mehrzahl der Fälle als gleich angegeben wurden, während bei denselben Versuchspersonen, wo bloß um gelegentliche Beschreibung gebeten wurde, die Punkte in der Regel ungleich erschienen. Daher kann man annehmen, dass die Suggestion, welche die Fragestellung des Experimentators in jenen Versuchen schließlich

ausübte, die zwei Punkte im allgemeinen als gleich erscheinen ließ. Dies stimmt mit der Beobachtung des Herrn Ar. überein, dass er seine Trugwahrnehmungen von seinen richtigen Wahrnehmungen zu unterscheiden vermöge. Aber sobald man versucht, diese Unterscheidung zu vollziehen, macht sich eine Suggestion geltend, und die Punkte werden gleich, wie mehrfach bei Herrn A. und auch anderen Versuchspersonen constatirt wurde. Oft erscheint ein Punkt mit einer mehr oder weniger diffusen Ausstrahlung nach einer Richtung hin, die der Versuchsperson aber genügend einheitlich vorkommt, um als ein Punkt bezeichnet zu werden. Anderemale erscheinen die Punkte gleichförmig, aber durch eine linienartige Empfindung verbunden. Zuweilen erscheinen sie als eine Linie. Wenn man ferner eine Hautstelle mit weicher Unterlage ganz leise mit einer Spitze berührt, so wird vielfach nur eine Empfindung erzeugt; sobald aber die Spitze tiefer einsinkt, wird die Antwort »zwei Spitzen« gegeben. Hier könnte man annehmen, dass der Vexirfehler durch das Dehnen der Haut oder andere physiologische Veränderungen erzeugt werde. Dagegen lässt sich jedoch bemerken, dass 1) bei einer solchen Berührung mit offenen Augen man zunächst eine diffuse, aber ziemlich gleichmäßig ausgebreitete Empfindung hat, die sich nur durch Autosuggestion als punktförmig vorstellen lässt, und dass 2) Vexirfehler eben so häufig auf Stellen zum Vorschein kommen, wo die Unterlage hart ist. Auf dem Knochen der Dorsalseite des Handgelenks und auf dem Fußknöchel erscheinen Vexirfehler ebenso häufig wie auf der Volarseite des Vorderarms. Aus dem Auftauchen des Vexirfehlers durch das tiefere Drücken der Spitze ist daher höchstens zu schließen, dass die von G. E. Müller¹⁾ hervorgehobenen Irradiationserscheinungen aller Tastempfindungen das Vorkommen von Vexirfehlern unter Umständen begünstigen können.

Ueber die Richtung, in welcher die zwei angeblich wahrgenommenen Punkte liegen, wurden folgende Versuche angestellt. Zunächst gab die Versuchsperson in 10 Versuchen die Richtung an, ohne Suggestion vom Experimentator; sodann wurden 10 Versuche ausgeführt, nachdem der Versuchsperson gesagt war, dass zwei Spitzen in einer bestimmten Richtung aufgesetzt würden. Bei den ersten

1) G. E. Müller, *Pflüger's Archiv*, Bd. 19, 1879, S. 217 f.

Versuchen waren die angegebenen Richtungen im Ganzen zwischen querer, longitudinaler und diagonalen Richtung von rechts nach links und von links nach rechts ziemlich gleich eingetheilt. Bei einigen Versuchsreihen wurde auch die Richtung überwiegend als quer angegeben, bei anderen aber als longitudinal oder diagonal. Das Ueberwiegen einer Richtung lässt sich durch eine beliebige Autosuggestion leicht erklären. Bei den Versuchen, bei denen der Versuchsperson vorher gesagt war, dass eine bestimmte Richtung inne gehalten werde, zeigte sich sehr deutlich der Einfluss dieser Suggestion. Wo die quere Richtung z. B. suggerirt war, wurden aus 26 Versuchen im Ganzen 16 mit querer Richtung angegeben: einmal wurde eine Linie in querer Richtung empfunden, 7mal zwei Spitzen in diagonalen, 2mal in »fast longitudinalen«. Bei diesen Versuchen zeigte sich auch oft die entgegengesetzte Wirkung, wie sie bei aller Suggestion möglich ist, indem die Versuchsperson unwillkürlich Autosuggestion ausübte. Aus einer derartigen Reihe von 15 Versuchen, wo die Längsrichtung suggerirt wurde, waren die empfundenen Punkte 6mal in querer Richtung, 3mal in longitudinaler (parallel dem Arm), 3mal in »fast longitudinalen«, 1mal in diagonalen von links nach rechts und 2mal von rechts nach links.

Auch über die Unterscheidung zwischen einem linienartigen und einem punktartigen Reiz ergeben sich aus den Versuchen von Herrn Franz einige interessante Beobachtungen. Es sei hier nochmals hervorgehoben, dass diese Versuchsreihen angefangen wurden, ohne der Versuchsperson irgend welche Nachricht über die Art oder den Zweck der Versuche mitzuthemen. Herr Fr. empfand zuerst bei einer Entfernung der Spitzen von 2 mm einen linienartigen Reiz. Er antwortete »eine Linie«, bis die Entfernung der Spitzen durch 2 mm-Abstufungen 48 mm war. Bei dieser Entfernung gab er an: »ich bin der Meinung, dass Sie mit einer Karte berühren«. Die Entfernungen wurden immer weiter bis zu 82 mm abgestuft. Manchmal schienen die zwei Enden des angeblichen Kartenrandes abwechselnd stärker gedrückt zu werden: solche Wahrnehmungen wurden von ihm als »rocky« bezeichnet und wir behalten den Ausdruck in dem folgenden Versuchsbeispiel bei. Die ersten Zahlen bedeuten die Entfernung der Spitzen in Millimetern. Die Richtung des Aufsetzens war stets longitudinal.

- 2 — eine Linie, Karte oder Aehnliches, etwa 2 oder 3 mm lang.
- 4 — ähnlich, aber punktförmiger.
- 6 — eine Linie, 2 oder 3 mm lang.
- 8 — eine Linie, 5 mm lang.
- 1 Punkt — eine Linie, dieselbe Länge.
- 1 Punkt — eine Linie, dieselbe Länge.
- 10 — eine Linie, länger.
- 12 — eine Linie, dieselbe Länge wie die vorige.
- 14 — eine Linie, 10 mm.
- 16 — eine Linie, 2 oder 3 mm.
- 18 — ein stumpfer Punkt.
- 20 — eine Linie, 5 mm, longitudinal (parallel dem Arm).
- 22 — eine Linie, 4 oder 5 mm, transversal.
- 24 — eine Linie, 10 mm, transversal.
- 26 — eine Linie, 5 mm, diagonal.
- 28 — eine Linie, 7 oder 8 mm, longitudinal.
- 30 — eine Linie, 7 oder 8 mm, diagonal.
- 32 — eine Linie, 7 oder 8 mm, diagonal.
- 34 — eine Linie, 7 oder 8 mm, longitudinal.
- 36 — eine Linie, 10 mm, »rocky«.
- 38 — eine Linie, 5 mm, longitudinal.
- 40 — eine Linie, 10 mm, longitudinal, »rocky«.
- 42 — eine Linie, 10 mm, longitudinal.
- 44 — eine Linie, 5 mm, longitudinal.
- 46 — eine Linie, 10 mm, longitudinal, »rocky«.
- 48 — eine Linie, 10 mm, longitudinal.
- 50 — eine Linie, 5 mm, longitudinal.
- 52 — ein stumpfer Punkt.
- 54 — eine Linie, 2 oder 3 mm, keine Richtung erkennbar.
- 56 — zuerst ein Punkt, wurde »rocky«.
- 58 — ein Punkt.
- 60 — eine Linie, 4 mm, diagonal.
- 62 — eine Linie, 2 oder 3 mm, diagonal.
- 64 — ein stumpfer Punkt.
- 66 — eine Linie, 5 mm, longitudinal, »rocky«.
- 68 — eine Linie, 10 mm, longitudinal, »rocky«.
- 70 — eine Linie, 3 mm, transversal.
- 72 — ein stumpfer Punkt.
- 74 — eine Linie, 5 mm, longitudinal, »rocky«.
- 76 — ein ausgebreiteter Punkt, keine Linie.
- 78 — ein stumpfer Punkt.
- 80 — mehr als zwei Punkte, »rocky«, mit deutlichen Enden.
- 82 — ein stumpfer Punkt.

Diese Versuchsreihe wurde 6 mal an verschiedenen Tagen wiederholt, mit immer demselben Resultat. Dass es sich hier um eine reine Autosuggestion handelt, lässt keinen Zweifel zu. Die Versuchsperson gibt an, dass sie die Vorstellung einer Karte aus den Empfindungen

allein bekam. Dies ist wohl annehmbar, weil die Uebergangsstufen zwischen der Empfindung eines Punktes und zweier Punkte, wie alle Versuchspersonen mehrfach angegeben haben, immer eine linienartige Empfindung ist. Die Angaben werden allen denjenigen überraschend sein, die den Einfluss centraler Apperceptionsvorgänge auf die Wahrnehmungen der Haut leugnen, wie auch denjenigen, welche die Wahrnehmung minimaler Distanzen auf Qualitätsunterschiede allein begründen wollen. Dass man einen Punkt oder eine Linie immer noch bei Entfernungen von 50—82 mm wahrnimmt, wo die Schwelle der Wahrnehmung zweier getrennter Punkte, wie spätere Bestimmungen zeigen, zwischen 30 und 45 mm liegt, ist vom Standpunkte dieser Erklärungsweisen unverständlich. Breiter konnte die Entfernung der Spitzen wegen der Kürze der Schenkel des Zirkels nicht gemacht werden, ohne die Spitzen schief aufzusetzen.

Aus diesen Versuchen wird ersichtlich, dass das scheinbare Erkennen der Zweiheit und der Richtung zweier Punkte, sowie des Unterschieds zwischen einem Punkte und einer Linie bei minimalen Distanzen größtentheils eine Sache der Autosuggestion ist. Man kann aber nicht behaupten, dass es ausschließlich so sei, weil der vorbereitende Einfluss einer diffusen Irradiation des Reizes sich immerhin noch nachweisen lässt. Was die Irradiation aber nicht zu erklären vermag, das ist die Punktförmigkeit, die Linienartigkeit und die Zweiheit der Empfindungen bei minimaler Distanz der Spitzen. Bei den kleineren Entfernungen der Spitzen, die unter der Schwelle liegen, scheinen diese der Versuchsperson erscheinenden Eigenschaften des Reizes fast, wenn nicht ausschließlich, aus reinen Autosuggestionen zu entstehen.

V.

Die mitgetheilten Beobachtungen bestätigen die Anschauung, dass das Ganze einer Wahrnehmung niemals mit dem Reize allein gegeben ist, sondern dass die Vorstellungselemente theilweise stets von Associationsverbindungen herrühren. »Es bleibt nur die Annahme möglich«, wie Wundt sagt, »dass durch jeden Sinneseindruck eine Menge von Dispositionen, die von früheren Eindrücken zurückgeblieben sind, in Miterregung geräth, und dass von diesen Miterregungen jedesmal solche in die neugebildete Vorstellung eingehen,

welche mit dem gegebenen Eindruck eine geläufige Vorstellung bilden können¹⁾. Die Erinnerungselemente, die in die neugebildete Vorstellung eingehen, sind durchweg nur Theile früherer Vorstellungen. Dabei werden, wie schon Scripture²⁾ festgestellt hat, die übrigen Elemente der Vorstellungen entweder nicht percipirt und bleiben außerhalb des Bewusstseins, oder sie werden percipirt, aber nicht appercipirt, indem sie in die undeutlichen Regionen des Bewusstseins gelangen, ohne irgend welchen Einfluss auf die neugebildete Vorstellung selbst auszuüben. Dagegen übt der Sinneseindruck auf die Erinnerungselemente eine verstärkende Wirkung aus, so dass sie von den durch den äußeren Reiz erweckten Empfindungen nicht mehr unterschieden werden können. Jede Wahrnehmung besitzt in diesem Sinne den Charakter einer Illusion oder Trugwahrnehmung, obgleich sie speciell nur dann so bezeichnet wird, wenn die so entstandene Vorstellung mit den Sinneseindrücken anderer Sinnesgebiete oder desselben Sinnesgebietes zu anderen Zeiten nicht vereinbar ist.

Damit die Versuchsperson durch die Tasteindrücke, die das Aufsetzen der Spitzen erzeugt, zwei Spitzen richtig wahrnimmt, ist es erforderlich, dass 1) die Tastempfindung mit früheren auf diese Weise erzeugten Empfindungen als gleich appercipirt wird, und dass 2) diese Empfindungen durch die Gesichtsvorstellungen ergänzt werden, die mit denselben in früheren Wahrnehmungen durch Association verbunden sind.

Aus den schon erwähnten Untersuchungen und Erörterungen von Scripture und Wundt wird ersichtlich, dass die Erinnerungselemente, die in eine Vorstellung eingehen, durch die Vorstellungen, die der Wahrnehmung unmittelbar vorangehen und dieselbe so zu sagen beherrschen, bestimmt werden. Stellt die Versuchsperson sich vor, dass entweder eine oder zwei in früheren Gesichtsvorstellungen enthaltene Spitzen auf die Haut aufgesetzt werden, und lässt sie die durch frühere Erfahrung gewonnene Berührungsassociation zwischen dem Tasteindruck und den Gesichtsvorstellungen entscheiden, ob sie eine oder zwei Spitzen wahrnehme, so entsteht eine Vorstellung, welche hinsichtlich des Verhältnisses der Tastempfindung zum Reizobject früheren Vorstellungen entsprechen wird. Stellt sie

1) Wundt, Philos. Stud. Bd. VII. S. 338.

2) Scripture, Philos. Stud. Bd. VII. S. 50 ff.

sich aber vor, dass entweder eine oder zwei Spitzen in die Wahrnehmungsvorstellung je nach den Qualitätsunterschieden der Tastempfindung eingehen, so wird eine Uebereinstimmung zwischen dem Tasteindruck und der Gesichtsvorstellung ganz und gar davon abhängen, ob die scheinbaren Qualitätsunterschiede der Tastempfindung von früheren Empfindungen thatsächlich den äußeren Reizen entsprechen. Stellt sie sich bei minimalen Distanzen vor, dass vielleicht oder wahrscheinlich zwei Spitzen auf der Haut ruhen, so wird die Möglichkeit einer Gesichtsvorstellung von zwei Spitzen lediglich davon abhängig sein, dass sich die Versuchsperson zwei Tasteindrücke, die untereinander in Berührungsassociation stehen, klar vorstellen kann. Wenn sie schon von vornherein an psychologische Analysen gewöhnt ist, wird sie dies sogleich vollziehen können. Falls sie sich jedoch mit der Analyse ihrer Empfindungen überhaupt nicht sehr viel beschäftigt hat, wird es erforderlich sein, ehe sie zwei Spitzen bei minimaler Distanz wahrnimmt, dass die Empfindungen derselben erst in Association mit der Vorstellung gebracht werden. Wenn aber dies einmal geschehen ist und die Aufmerksamkeit der Versuchsperson auf den Tasteindruck allein gerichtet wird, dann sind die Bedingungen für eine plötzliche Abnahme der scheinbaren Schwelle vorhanden, wie dies bei den Herren Hef., Ar. und P. schon erwähnt wurde. Bei Herrn Hef., der sich am wenigsten mit psychologischen Untersuchungen beschäftigt hatte, war diese Abnahme am deutlichsten: zunächst steht Herr P. und in dritter Reihe Herr Ar., der sich zwar viel mit Psychologie beschäftigt, aber auf Tastempfindungen bis dahin wenig seine Aufmerksamkeit gerichtet hatte.

Es ist aber doch nicht zu erwarten, dass solche Versuchspersonen bei jedem Reiz, welcher Art er auch sein mag, zwei Punkte empfinden werden. Dies kann nur dann vorkommen, wenn die Versuchsperson durch Gewohnheit, psychologische Bildung oder sonst irgendwie die Uebertragung der Aufmerksamkeit vom Reizobject auf die subjective Empfindung von Anfang an vollzogen hat. Wo sie eine »Einübung« gebraucht, bevor diese Uebertragung geschieht, wird das Urtheil für einen Zeitraum, in welchem die sogenannte Verkleinerung der Schwelle stattfindet, dem Associationsgesetz unterworfen sein. Diese zwei Fälle müssen deutlich unterschieden werden. In dem ersten stellt sich die Versuchsperson von Anfang an zwei Spitzen deutlich vor: im zweiten

geschieht dies nur als ein Product der »Einübung«. In jenem Falle kennt die Versuchsperson den Vorgang, durch den sie ihre Wahrnehmung vollendet hat; in diesem ist es in der Regel unmöglich, eine Beschreibung dieses Vorgangs anzugeben. In jenem handelt es sich um einen irreführenden Versuch, die Tastempfindung reflectiv zu analysiren; in diesem um einen Assimilationsvorgang¹⁾ unter Bedingungen, wo die Qualitätsunterschiede innerhalb der einzelnen Empfindung nicht genügend sind, um eine sichere Unterscheidung der Reize zuzulassen. Auf diese Weise ist es theilweise erklärlich, dass eine Spitze bei solchen Versuchspersonen bald als eine, bald als zwei wahrgenommen werden kann. Fängt man eine Versuchsreihe mit dem Aufsetzen einer Spitze an und setzt die Versuche dann mit zwei Spitzen in Abstufungen von 1 mm fort, so wird die Versuchsperson wahrscheinlich »eine Spitze« antworten, bis die Distanz 4 oder 5 mm beträgt. Bei dieser Entfernung wird die Empfindung »länglich« oder »ausgedehnt« (wie die Aussage häufig lautet); durch die Association dieser Empfindung mit der eines anderen Punktes wird diese nun in der That hervorgerufen. Sobald die Entfernung der Spitzen aber 7 oder 8 mm wird, hat die entstehende Empfindung mit der eines Punktes keine Aehnlichkeit mehr, und in Folge dessen kann nur wieder die Antwort »eine Spitze« erfolgen, falls nicht die Ueberzeugung, dass es eigentlich zwei Spitzen sein sollen, auch hier die Association hervorruft. Ist die Entfernung 20 oder 30 mm geworden, so werden nun zwei Spitzen wirklich wahrgenommen. Man erhält so die Erscheinung von zwei Schwellen, einer unteren und einer oberen. Bei fünf Versuchspersonen wurde diese Erscheinung bemerkt: bei Herrn A. am deutlichsten. In allen diesen Fällen wird die durch Association hervorgerufene Empfindung durch den Apperceptionsact in der Einheit einer Vorstellung zusammengefasst. Dann gibt der Tasteindruck selbst der ganzen Vorstellung die Stärke und Lebhaftigkeit einer wirklichen Wahrnehmung, und die Trugwahrnehmung ist fertig.

Dieser Process scheint namentlich den Erscheinungen in den Versuchen der Herren Hef., A. und P. zu Grunde zu liegen. Bei

1) Der Name Assimilation wird im Sinne Wundt's gebraucht, um die Verbindung eines Sinneseindrucks mit ihm gleichen Erinnerungselementen früherer Vorstellungen zu bezeichnen. (Philos. Stud. VII. S. 335.)

der Versuchsperson trat bei der Wahrnehmung minimaler Distanzen in der Urtheilsbildung ein Assimilations- statt eines Complicationsvorganges ein: dies zeigt sich zunächst in der Richtung der Aufmerksamkeit auf den vom Reizobject unabhängigen Empfindungsinhalt und in der Beseitigung der sonst gewohnten Rückbeziehung der Tastindrücke auf Gesichtseindrücke. Die Versuchsperson ist gar nicht im Stande, ihre Trugwahrnehmungen von richtigen Wahrnehmungen zu unterscheiden. Beide sind Apperceptionsvorgänge; aber während es bei der normalen Apperception keine dem Subject bewusste Pause zwischen dem Reiz und der Wahrnehmung gibt, ist sich bei der Trugwahrnehmung jede Versuchsperson einer solchen Pause bewusst. Ebenso gibt es dort keine bewusste Analyse des Empfindungsinhaltes; hier gründet sich das Urtheil auf eine solche, wodurch es schließlich zu jener auf Assimilation beruhenden Autosuggestion kommt.

Es sei hier kurz bemerkt, dass die Autosuggestion auch in anderen Gebieten eine große Rolle spielen kann. Fast überall, wo die Methode der Minimaländerungen angewendet wird, kann ihr Einfluss vorkommen. In keinem anderen Gebiet ist aber wohl dieser Einfluss so groß wie in dem des Tastsinnes. Will man denselben vermeiden, so ist es nach den Ergebnissen unserer Untersuchung vor allem erforderlich, die Aufmerksamkeit der Versuchsperson vollkommen objectiv und das Urtheil rein sensorisch zu erhalten. Dazu genügt es aber entschieden nicht, die Methode unwissentlich anzuwenden; dadurch wird vielmehr der Spielraum der Autosuggestion nur größer gemacht, wie die Versuche bei Herrn Franz zeigen. Die Versuche bei den Herren Mosch und Franz beweisen, dass eine vollkommen wissenschaftliche Anwendung der Methode am günstigsten war; dabei ist es aber erforderlich, dass die Versuchsperson bei minimalen Distanzen sich von aller Anstrengung, die Schwelle sehr genau anzugeben, möglichst frei hält. Es sollte ferner keine erhebliche Pause zwischen dem Reiz und dem Urtheil stattfinden, und jede Reflexion der Versuchsperson vermieden werden. Wo diese Bedingungen nicht durchaus innegehalten werden, können die Resultate nicht sicher als vertrauenswerth angesehen werden.

Von diesem Standpunkt aus seien an letzter Stelle einige als Beispiele herausgegriffene Arbeiten dieses Gebietes einer kurzen Kritik unterworfen.

Es wurde schon von Czermak¹⁾ bemerkt, dass die Schwelle für die Wahrnehmung zweier gleichzeitig gereizter Punkte der Haut bei Blinden und bei Leuten, deren Beruf eine besondere Uebung des Tastsinnes erforderlich macht, wie z. B. Schriftsetzern, kleiner ist als bei andern. Das Beispiel der Schriftsetzer ist aber für die Einübungstheorie deswegen nicht ohne weiteres beweiskräftig, weil das Erkennen der Buchstabenformen sehr wohl ein Assimilationsvorgang sein kann. Untersuchungen bei Blinden zeigen in der Mehrzahl der Fälle, dass dieselben eine kleinere Schwelle angeben als Sehende. Die Resultate Czermak's sind seitdem von Goltz²⁾, Gärtner³⁾, Heller⁴⁾ und Miss Washburn⁵⁾ bestätigt worden. Es wird aber von Heller bemerkt, dass der Unterschied zwischen den Sehenden und den Blinden in dieser Beziehung entschieden nicht so groß ist, wie früher angenommen wurde, und Uhthoff⁶⁾ konnte sogar keine feinere Schwelle bei den Blinden als bei den Sehenden finden.

Bezüglich der von Henri und mir früher veröffentlichten Versuche über den Vexirfehler sei noch Folgendes erwähnt: Wir sind damals zu dem Schluss gelangt, »dass die Wahrnehmung zweier Punkte bei der Berührung eines Punktes der Haut zunächst von physiologischen Bedingungen (wahrscheinlich den Nervenverbindungen des berührten Punktes) abhängt, dass sie aber durch psychische Vorgänge, wie Wissen und Erwartung, beeinflusst wird«⁷⁾. Der erste Satz dieser Schlussfolgerung wurde aus den folgenden Thatsachen gezogen: es wurden zwei Punkte A und B auf dem Arm der Versuchsperson mit einer Spitze berührt. Aus den Tabellen I und II ist ersichtlich, dass 1) die wahrgenommenen Punkte einer Versuchsperson, die bei der Berührung von A auftraten, 25 mal in querer und nur 5 mal in longitudinaler Richtung zu sein schienen; bei der Berührung von B schienen sie aber nur 14 mal in querer und 20 mal in longitudinaler Richtung zu sein. Die Frage erhebt sich nun, warum

1) Czermak, Sitzungsber. d. Wiener Acad., 2. Abth. XVII, S. 563; Molesch. Unters. I, S. 188.

2) Goltz, De spatii sensu cutis. Königsberg 1858, S. 9.

3) Gärtner, Zeitschrift für Biologie. 1881. S. 56.

4) Heller, Philos. Stud. Bd. XI S. 226 f.

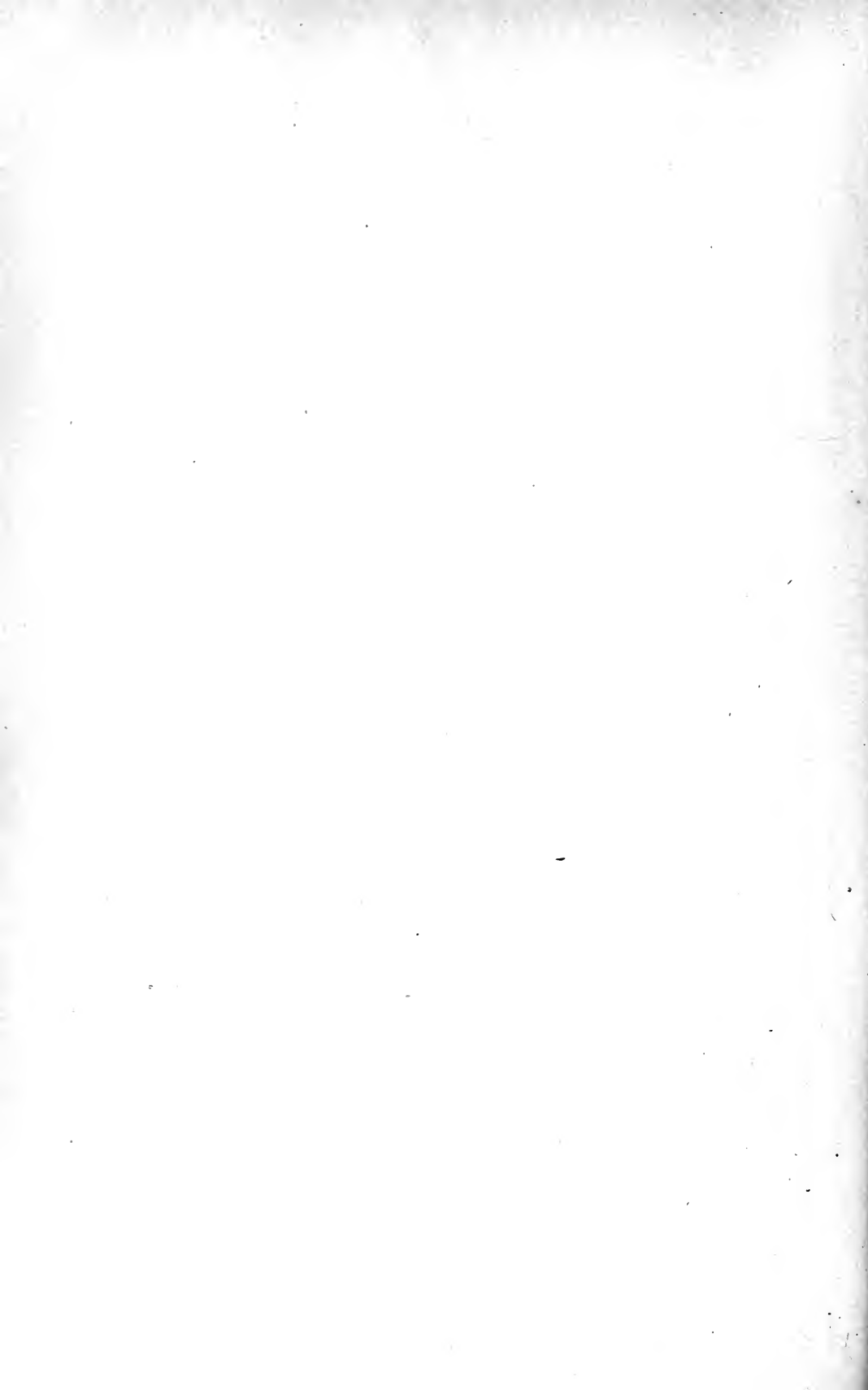
5) Miss Washburn, Philos. Stud. Bd. XI. S. 191 f.

6) Uhthoff, Untersuch. über das Sehenlernen eines siebenjährigen blindgeborenen und mit Erfolg operirten Knaben. Hamburg und Leipzig 1891. S. 54.

7) Henri und Tawney, Philos. Stud. Bd. XI. S. 435.

die Punkte bei der Berührung von A meistens in einer Richtung, bei der Berührung von B aber meistens in der anderen Richtung zu liegen schienen. In Bezug auf die Gleichheit oder Verschiedenheit erscheinen die Punkte bei der Berührung von A 10 mal gleich und 21 mal verschieden, bei der Berührung von B aber 23 mal gleich und nur 12 mal verschieden. Wie sind diese Regelmäßigkeiten entstanden? Es wurde vorausgesetzt, dass die berührten Punkte der Versuchsperson nicht bekannt waren. Mehrere Punkte auf dem Arm wurden mit Anilin markirt, damit A und B von dem Subject nicht besonders bemerkt werden konnten; und es wurde als unmöglich angenommen, dass die Regelmäßigkeiten durch irgend einen psychischen Vorgang entstanden seien, weil die Versuchsperson nichts von den Punkten wissen konnte. Jeder Punkt musste daher mit andern neben ihm stehenden Punkten in irgend welcher physiologischer Verbindung stehen, um diese Regelmäßigkeiten hervorzubringen. Dieser Schluss scheint richtig, wenn zugegeben wird, dass die Versuchsperson thatsächlich nichts von den berührten Punkten wisse¹⁾. Schon aus allgemeinen Gründen aber, insbesondere aus der Thatsache, dass die Punkte A und B weit genug entfernt waren, um eigenthümliche locale Färbungen zu besitzen, ist zu vermuthen, dass die Versuchsperson doch manchmal denselben Punkt bei einer zweiten Berührung wiedererkannte. Ich hatte bei diesen Versuchen ziemlich oft den folgenden Gedanken: das ist derselbe Punkt, bei welchem ich früher zwei Reize in querer Richtung wahrnahm. Dann suchte ich, ob nicht wieder Punkte in derselben Richtung vorhanden seien, und sehr oft erschienen die zwei Empfindungen nach einiger Zeit. Der Autosuggestionsvorgang ist hier ganz deutlich. Auch von der Methode lässt sich vielleicht sagen, dass durch die Fragen, die an die Versuchsperson nach jedem Versuch gestellt wurden, die Aufmerksamkeit derselben zu sehr auf Nebenerscheinungen gerichtet und dadurch der normale Verlauf der Wahrnehmungen gestört worden sei. Ueberhaupt dürften diejenigen Angaben der Versuchspersonen die nützlichsten sein, die spontan, ohne suggestive Beeinflussung von Seiten des Experimentators gemacht werden.

1) Die Bemerkung des Herrn Judd (Philos. Stud. XII. S. 447) scheint auf einem Missverständniss dieser Folgerung zu beruhen: »physiologisch« heißt nicht nothwendig »peripherisch«, wie er vorauszusetzen scheint.



THE NEGATIVE IN LOGIC.¹

BY PROFESSOR A. T. ORMOND.

Historically the negative has occupied the attention of logicians since the first beginnings of the science. Aristotle gave it a prominent place in his reflection and in modern times it has been discussed by all the masters in this field; by Leibnitz and Kant, by Hamilton, Lotze, Sigwart, Wundt, Bradley, Bosanquet and Benno Erdmann. It is not my purpose in this paper to review the work of these thinkers, even in outline, but rather with their results in mind to attempt a statement of what I conceive to be the most important features of an adequate theory of logical negation. In the first place it is clear, I think, that the logical negative is very closely implicated in the general theory of judgment and that a radical treatment of it must go to the roots of judgment itself. For this reason a considerable section of this paper will be devoted to judgment with a view to seeking its psychological and logical grounds.

In treating judgment psychologically we must conceive it as a conscious function, and this, followed back to its very first presupposition, would involve the question of the origin of the consciousness in which the function arises. But whatever the responsibility of psychology may be for the genesis of consciousness, logic is free I think to assume the medium in which the functions it is interested in are found. A question, however, which does, indirectly at least, concern the foundations of logic

¹ Read in abstract at the Boston meeting, American Psychological Association. The discussion is mainly psychological.

is that of the organic conditions in which consciousness operates. The tendency of the genetic thinking of the time is to go back of the psychological to the biological in order to discover the first laws or conditions of conscious activity. And this is, I think, on the whole, a healthy disposition, inasmuch as the vital and the psychic activities cannot be separated in an organism which has once become the bearer of consciousness. I do not mean that there are none of the activities of such an organism that are not psychic, but that within the circle of the conscious, the vital and the psychic are one and the same. To omit all detail, the important question here is, how, for our purposes, shall the relation of the biological to the psychic be conceived? There are two view-points that are to be kept separated in our thinking: the external or physical and the internal or psychic. From the physical standpoint, which is the biological, we view consciousness as something in the organism and superadded to the organic functions, whereas, from the psychic point of view, which is that of mind itself, consciousness is not simply in an organism or an appendage to its activities, but it is a comprehending term, the medium in which the existence of everything is realized, and in which the organism, in order to get itself recognized among existent things, must somehow become immanent as part of its content. Realizing this point of view we will be led to regard that duality which constitutes the mould of organic activity in general, the interaction of organism and environment, as immanent and structural in the sphere of conscious activity, and from the same point of view the biological laws of habit and accommodation will become immanent laws of consciousness. I mean by this that consciousness does not simply contemplate the biological functions as external and conditional to its own activities, but that when conscious activity arises, say, as will, the laws of habit and accommodation are taken up into consciousness and become constitutional principles of volitional activity.

Assuming, then, that the vital conditions the psychic in its own sphere by becoming immanent and constitutional to it, our notion of psychic activity will resolve itself into that of *conscious* vital function transforming and yet obeying the life categories

which have been taken up into the conscious sphere as immanent laws of psychic activity. Nor will it be difficult from this point of view to realize the ground on which the first conscious activity may be characterized as volitional, inasmuch as it will take the form of conscious reaction of the organism upon its environment, which, operating under the laws of habit and accommodation, it gradually assimilates and absorbs into itself. The general concept which I have sought to emphasize at this point is the immanence of the vital from the psychic point of view and the consequent necessity for translating the biological categories into internal and structural principles of the conscious activities.

If we regard the conscious organism simply as acted upon by its environment, that is, as a recipient of stimulations, there is no ground for ascribing will to it. It is only when we conceive it as active and as reacting upon the ground of stimulation that we can think of it as will. What we call will can, in these early psychic activities, be nothing but the conscious responses by which the organism effects its assimilative and adaptive movements. We may call them *pulses of self-assertion*, by which the organism wrecks itself upon the ground of stimulation, and the acts will be acts of self-conservation and will fall under the general category of survival.

These earlier acts of volition will not be primarily motivated by any idea or representation, but rather by some feeling of pleasure or pain, most likely one of pain, since mere pleasure feeling could not serve as a motive for activity, but, on the contrary, in itself and without some accompanying idea or representation which would translate it into teleological terms, would tend to arrest motion. If we assume then that the very first motive-impulse, logically considered, is painful feeling we may conceive the primal impulse of volition as some want or unsatisfactory condition which *impels* the conscious organism to escape from its present state into one that shall be less intolerable. We have then the conception of a will motivated in a negative sense from behind, but, so far as this element of motivity is concerned, blind as to what is before it and having no other guidance than the specific quality of the painful impulse, to enable it to pick its way among

the pleasant and painful stimulations of the environment. But by the pain-motive, which is a principle of *avoidance*, we may conceive the organism as feeling its way with a certain degree of selective intelligence, it being understood that the pleasure-motive becomes also active, and that representation when it arises attaches itself to both pleasure and pain as a teleological principle of positive and negative selection.

The will of such an organism would be an active function of appropriation and avoidance moving under the guidance of the selective motives, and the special question which arises here from our point of view is how the volitional activity comes to take on an intellectual character and become what we call judgment. To answer such a question in detail would involve a wide excursion into genetic psychology. The following statement must suffice at this point. The conscious organism not only collides volitionally with the grounds of its stimulations, but out of these collisions arise representations (the spatial no doubt arising first) which are to be conceived as elements of form under what these grounds appear to us as *objective* and *intelligible*. Let us suppose this process as completing itself in the presentation to consciousness of the objects of a world in the midst of which its functions are performed. So that what was blindly and vaguely realized before through feeling now stands out in a representation. If from this representation we subtract the volitional pulse we have simply a world presented but not affirmed. But if we restore this pulse as a conscious reaction upon the presentation we have the simplest assertion of the object; that is, judgment in its most elemental form. This is essential. In all judgment the central thing is a volitional pulse. To this as genus certain differentiae must be added in order to constitute judgment, and the next section must be taken up with a determination of these differentiae.

From the genetic point of view there are originally only two kinds of judgments: *existential*, or judgments which assert simple existence, and *relational* judgments, which assert relations among existents. The elements of the existential judgment are: (1) the *objective* representation of something to consciousness; (2) the act of *positing*, which is virtually our willing the ex-

istence or non-existence of the thing. But between the representation and the volitional fiat, let this thing be, there must intervene the motive of the fiat, which is some interest. This interest must coalesce with the representation in order that the volitional pulse may be stirred to utter itself in the let this thing be or not be. Thus arises the simple existential judgment. It is more than mere perception; we must perceive and then do something to our perception before the content or object may exist to and for us. Interest must fall upon the object represented, and there must be that pulse of self-commitment which has been translated into the let this thing be, before the judgment of existence can be said properly to arise. The judgment thus puts a kind of personal stamp of endorsement on the object of perception.

The relational judgment is more complex. Its prototype will be found in volitional alternation, or that process by which the animal or the young child selects out of conflicting, or at least competing means, those which will serve its end. Thus the chick, whose end is food and whose alternatives are cinnabar caterpillars and other caterpillars, will choose the other caterpillars, rejecting the cinnabar species. This process ceases to be purely volitional and takes on judgmental complexion when the alternatives are consciously conceived, or become related in thought as alternative means of satisfying the volitional end; that is, when a body of experience or knowledge becomes the guiding principle of selection. In the chick's case the selective principles are all below the level of *thinking*. The end, food, although not conceived in any intellectual terms, yet functions in the chick's consciousness as a limiting and guiding principle. The chick's universe is one of food, and the included alternatives are food-alternatives. The body of experience acquired by the chick thus conditions its selective activity. The motives of selection rise to the plane of thinking when they themselves become the objects of representation. The child performs a judgment of relation when it pronounces an object good or selects it because it is good. In such an act the relation of the object to some end sought by the child is seized and affirmed. This is the simplest kind of judgment of relation. The more com-

plex forms arise when the less obvious relations on which classifications proceed come into consciousness. In this progress the immediate relations of the object to the survival of the subject gradually drop into the background and the activity takes on a more purely intellectual form. The principal differentia of the judgment of relation may then be stated as follows: (1) a body of experience or knowledge which determines the sphere or universe of existential relations; (2) the appearance in this universe of a number of competing alternatives whose relations to some interesting end also rise into consciousness and specifically determine the judgment. These are the differential features of the act in which the volitional pulse of assertion is central and which takes the form of appropriation or rejection of some among the included alternatives.

Now, it may be asked at this point, do we not beg the question when we postulate a universe which includes all the alternatives as a *condition* of the judgment of relation? How else, it may be asked, than through a process of judgment could such a universe arise? We answer that our first universes arise in perceptual experience. Judgment is never without presuppositions. The chick no doubt learns from experience what objects are food for it before it is able to select among the objects presented to it. Our logical universes may be and no doubt are in the later stages of experience, products of logical processes. But this is evidently not the case at the point where judgment first arises. The first universe must be one that is supplied by extra-logical experience. When arrived at, however, the judgment function will operate within it in the manner indicated.

We have then the two distinguishable types of judgment—the Existential, which asserts simply existence, and the Relational, which is more complex and selects among alternatives included in a broader genus or universe of existent relations. If, now, we leave the first species out of view as being for our purposes relatively unimportant, we may say that the judgment function is a *disjunctive operation within a larger genus or universe*. Bosanquet recognizes this character in his doctrine that every judgment involves as its presupposition a larger comprehending judgment. The comprehending term need not,

however, as we have contended above, be a judgment. It may be some body of experience which for the time being functions as the real subject of judgment. In other words, the ground of the disjunction may be purely psychological.

To summarize the discussion up to this point, judgment rises out of volitional grounds. It is informed by the motive of volition and it includes the volitional pulse as its central essence. All judgment then is volitional in its nature. The volitional pulse becomes a pulse of judgment when a field of representation arises to which some interest attaches. The simple judgment of experience is the first result. The subject of a logical judgment need not be logical; it may be purely psychological, a body of extra-logical experience. The judgment of relation preserves the volitional character and simply adds other differentia. We have seen that the ground of this judgment is a genus which comprehends, and at the same time limits, the alternatives about which the judgment is pronounced, and that the judgment itself is essentially disjunctive. But the comprehending genus need not be a judgment; it may be psychological and not logical. Now this conclusion is the one that might be anticipated from the point of view of logical immanence.¹ For just as the vital is immanent in the psychic so in the region of the logical processes the later comprehends the earlier which acts as its inner motive and the psychological is immanent in the logical. We see at this point how the psychological universe, which is the concrete universal of the Lotzean school of logicians, becomes the immanent motive and spring of logical processes, so that it is not necessary to postulate an infinite series of logical universals, but experience passes by insensible gradations from the pre-logical into the logical stage.

The fact that every judgment either affirms or denies led Aristotle to regard affirmation and negation as coördinate moments in judgment. Modern logicians have tended rather to subordinate negation to affirmation, and some have gone so far as virtually to deny the reality of negation. Without delay-

¹The doctrine of immanence set forth here is not identical with Erdmann's. What it means is the internal activity of psychological content as a motive in logical processes.

ing on historical details, however, we may seek an answer to two questions concerning the negative: (1) How does negation arise? (2) What is its function in judgment? If we bear in mind the relation of judgment to volition we will be ready to agree, I think, that all judgment is positive. There cannot be a judgment in which something is not asserted. All judgment is, therefore, positive and assertative. The distinction between affirmation and denial must then be a distinction between two kinds of positive assertion. So much seems clear. But it is not so clear what an assertion that is neither affirmative nor negative can be or how such assertion can be real. How shall we render the question intelligible? In the first place, it is clear, I think, that when I say in a negative existential judgment, for example, that no griffin exists, the nominal subject, griffin, is not the real subject which motives the assertion. The real subject is something known; some conception of reality which necessitates the denial. Now the assertive force of the judgment lies in the self-conserving force of this backlying knowledge or conception which simply maintains itself against what is incompatible with it. Every existential judgment may then be regarded as the self-assertion of its real subject, *pro* the compatible, *contra* the incompatible. There is always a positive; the self-assertion of the real subject which conditions both affirmation and negation. Suppose, for illustration, that this were not the case and that griffin were the real subject of the judgment, the function of the denial would be to remove its own subject and thus commit logical suicide. The real subject is that which necessitates the denial and is some backlying knowledge or conception of reality which is incompatible with the existence of griffins. The real subject maintains itself against its incompatible. Thus the negative judgment arises. We see, then, how the negative existential judgment rests on position. It is not pure destruction and removal, but something establishes itself in and through it.

If we take the judgment of relation the same fact comes out even more clearly. We have seen that the judgment of relation is disjunctive and that it presupposes a genus or universal that is either logical or psychological. Take the judgment:

men are not infallible. Here the real subject which necessitates the denial is some backlying knowledge or conception of human nature with which the notion of infallible men is incompatible. Let us suppose that this is not the case, and see what follows. The denial simply sweeps away the notion of infallible men and leaves nothing behind. There is thus no motive for further progress. We can escape this irrational result only by identifying the real subject of the judgment with the knowledge or conception of reality that necessitates the denial. This need not be a definite affirmation that men are fallible or even the knowledge of that fact, but rather some knowledge or conception of human nature that is incompatible with its infallibility. This real subject it is that asserts itself in every judgment and renders it positive, whether its form be affirmative or negative. And this it is, and this alone, which enables judgment to make progress through denial as well as through affirmation.

How, then, are affirmation and negation related? The answer cannot be given without recognizing the position of the real subject as the condition and motive of both affirming and denying. Some logicians, as Sigwart and Bradley, take the ground that a negative judgment presupposes an affirmation or an attempted affirmation of the opposite. This is also substantially the view of Benno Erdmann. But it is clear at this point, I think, that what the denial does presuppose is the position, the self-assertion of the real subject. The real subject maintains itself and necessitates the specific denial or affirmation, as the case may be. This real subject is always related to the judgment as the genus or universal within which the affirmation or denial falls. It is this larger assertion, and not a specific affirmation of the thing denied, that is necessarily presupposed in the negative judgment.

In what sense then are affirmation and denial related to each other? We do not inquire here what the actual relation between any two given affirmations and denials may be, for a denial *may* be the contradiction of a previous affirmative assertion; but rather what is the essential and necessary relation between affirmation and denial *as such*? If we bear in mind that it is the real subject that necessitates the judg-

ment which is in its nature, an appropriation of what is compatible or a rejection of what is incompatible with its actual content, it would seem not to be necessary that even proposed or suggested affirmation should precede denial, as is the contention of Bradley and Bosanquet. For if the real subject which is the genus or universe within which the judgment functions, necessitates the affirmation or denial on the ground of compatibility or incompatibility, it would seem to follow that inasmuch as the relation of incompatibility may be directly apprehended, like the inequality of two lines, therefore denial may be direct and unmediated by any suggested or attempted affirmation. It seems gratuitous for us to say that we cannot deny without having first gone through the form of affirming. Limiting the implications of the relation to the requirement of necessity, I cannot see any sufficient justification for the doctrine that negation is mediated by affirmation. On the contrary, so far as the logical relations of the two moments are concerned, they seem to be perfectly coördinate. The real subject approaches the alternatives contained in the limiting genus without logical prepossession and affirms or denies them with direct relation to itself and without regard to their relations to one another.

But from another point of view there is a difference. We have seen that the real subject gets on through both affirmation and denial. But it gets on directly by affirmation, while its progress through denial is only indirect. The organism maintains itself through the avoidance of what is hurtful, as well as by the assimilation of what is beneficial; but the two functions do not advance it in the same way. It is directly benefited by food, but only indirectly and mediately by the avoidance of the hurtful. The same is true of affirmation and denial. While logically they are coördinate in the sense that neither is mediated by the other, yet affirmation ministers more directly to its subject than does negation. Naturally, then, the interest in affirmation will be stronger than that which attaches to denial and, therefore, psychologically, if not logically, negation will be forced into a secondary place.

To the question, then, of the necessary relation of affirmation and negation we answer that logically they are coördinate

and inconvertible modes of assertion, and that the real subject of discourse advances through denial as well as through affirmation. The first part of this conclusion seems, however, to be contradicted by double negation, which by common consent of logicians is held to be identical with affirmation. Now, it is true that the denial of a denial leads up to an affirmative judgment. But this is not the same as to say that double negation and affirmation are identical. The truth is the denial of a denial simply sweeps the first denial away and leaves the ground clean for an affirmation which immediately follows. But this affirmation is a third judgment. That this is true will appear not only from an inspection of the movement of thought in such cases, but also from the consideration that a denial of a denial contradicts it, and leads, therefore, by a process of immediate inference to the assertion of the contradictory affirmative. Double negation is not affirmation, then, but simply prepares the way for affirmation by destroying the negative that blocks its path. It is one of the modes by which the real subject necessitates an affirmative judgment. Logical analysis thus fails to lend any support to the idea that affirmation and denial are not perfectly distinct mental functions, or that there is any point where they tend to lose their difference and become identical.

There has been great difference of opinion among logicians as to whether the negative ought to be referred to the predicate or to the copula of a judgment. If we distinguish at all between predicate and copula, which seems to me to be a doubtful performance, then the predicate will be the name of something that is conceived to affect the subject in some way and the copula will stand for the mode of this affection. The copula may then be regarded as a conceived relation between the subject and the predicate matter. Let us take the judgment, men are not fallible. If the negative belongs to the predicate, then, as Benno Erdmann points out, the judgment becomes affirmative, men are non-fallible and the distinction between affirmation and negation is virtually abolished. But if it belongs to the copula the negative maintains itself and a certain conceived content is rejected by the subject on the ground of incompatibility. Benno Erdmann holds that the negative is to be referred to the copula,¹

¹ *Logik*, Erster Band, § 57, 348.

and in this I think he is unquestionably right, and would only take issue with him on the point that what the negative copula sweeps away is an affirmation or a proposed affirmation. It has been shown, I think, that all that is necessarily involved in negation is the presence of an alternative that is equally open to affirmation or negation and that whatever more than this may be involved in any given case must be determined by the context of the judgment. The denial removes an alternative that might have been affirmed had it been compatible, and it removes it as a whole out of the sphere of possibilities. The function of denial is thus always removal, sublation.

The *implications* which the negative judgment may contain is a consideration that is to be carefully separated from that of the meaning of denial. The implications of the denial when it has once performed its function are to be determined in view of the relations of opposition which subsist between it and other conceivable judgments involving the same terms. Denial contradicts affirmation in the sense of wiping it out completely. This insight is as old as Aristotle. But when the denial is taken as a judgment form then it stands related differently to other judgment forms, affirmative and negative. Thus if we say all regular students are eligible to college honors, we, in effect, say that any regular student is eligible. The denial of this implies that there are regular students who are not eligible. This gives the traditional opposition of all are and some are not. But the negative judgment may be denied by others which are not contradictory. Thus all are will be denied by none are. But it is not necessary to enlarge on a topic so familiar. The important point of the discussion here is the necessity for distinguishing between the meaning of denial and the implications of the judgment in which the denial is incorporated.

Benno Erdmann's doctrine of the immanence of the predicate in the logical subject makes it possible for him to speak more profoundly than most logicians on the subject of negation. Every denial, he says, rests on the failure of immanence of the predicate in the subject.¹ This is true. But Erdmann does not, I think, develop the full implication of his own doctrine. If we

¹ Logik, Erster Band, § 57, 353.

take the subject in the narrow technical sense in which it is ordinarily used, then no question of immanence can arise and the denial simply sweeps away a possible synthesis, leaving nothing behind. Thus if we deny that men are infallible we remove the notion of infallible men and leave nothing in its place. Thinking is thus brought to a standstill with no motive for any further progress. In order to avoid such a disaster the real subject of the judgment must be something that survives both affirmation and denial. It must be some universe or piece of knowledge lying in our consciousness which asserts itself in the removal of the incompatible or in the assimilation of the compatible. The real subject survives the denial and gets on by means of it. And it is this subject alone which has immanent in it all the real alternatives on which affirmations might be founded, while denial in such a case indicates a failure of immanence in the sense that what it denies is no real alternative at all. It does not exist within the confines of this larger subject. This amendment I would suggest to Erdmann's doctrine of the immanence of the predicate in the subject; an amendment that would be perfectly consistent with his refusal to allow to denial any independent significance. Denial always, on this view, points back to a larger self-asserting subject, in relation to which it is the cancellation of an affirmative possibility, and although it does not as definitely point forward to affirmation as Erdmann thinks, it does, in fact, prepare the way for the more definite self-assertion of the real subject.

The fruitful question regarding the function and value of the negative is, as Bosanquet says, why in knowledge we cannot do without denial? A full answer to such a question is, perhaps, impossible. But if we have rightly conceived the relation of judgment to the volitional processes which underlie it, an equivalent question would be, why cannot volition do without rejection? The obvious answer here is that the environment contains things that are incompatible with the organism's survival. And just as we have reason to think that pleasure without pain could not supply an adequate stimulus to volitional activity, or a principle of selection that would enable it to avoid the hurtful, so for analogous reasons we have grounds for thinking that knowledge

could not get on with simple affirmation. The infinite sphere of alternatives that may confront any given subject will contain the incompatible as well as the compatible. Now, before the incompatible, affirmation is powerless. There is needed a selective principle which will enable the subject to assert itself against and in spite of the incompatible. Hence the necessary function of negation. Knowledge makes progress as much by denial as by affirmation. But it progresses in a different way through denial, and at no point can the two modes be identified.

Denial we have seen to be a selective principle in the activity of knowing. In practice, however, it possesses various degrees of selective value. To begin at the bottom of the scale the value of negation is at its minimum in what Kant has called the infinite judgment. This judgment definitely assigns the negative to the predicate of the judgment. When we say, for example, virtue is not four-cornered, we assign virtue to the infinite universe of non-four-cornered things where it has stones, vegetables, caterpillars and other things for its companions. The negative is at its lowest terms here because it is most indeterminate; it has simply expelled virtue from the province of four-cornered things, but otherwise leaves it to wander at large in an undetermined universe. If we leave out of view the infinite judgment and connect denial where it properly belongs, with the copula of the judgment, its value will be found to vary indefinitely. Its function is uniform, the removal of a false alternative, but what this removal does for knowledge is variable. The point on which I wish to put emphasis in this connection is that the significance of negation will, other things being equal, vary with the extent and richness of the real subject, which necessitates the denial. The denial of the scientist means more for knowledge than that of the unlearned, though both denials be equally valid. The denial of the child is less significant than that of the man. The savage looks out on the stars and shakes his head; the trained astronomer, looking through his telescope, makes the same sign. The difference in significance is vast, and why? Because the denial is necessitated by knowledge, and just in proportion as this knowledge is rich and exact will the denial be definite and specific. The astronomer's denial, perhaps, brushes aside a

false hypothesis, or removes the only obstacle in the way of a great discovery, while that of the savage signifies, it may be, only the failure of some combination which has a superstitious import to his mind. Some of the later writers on logic represent this tendency of negation to become more specific as approximation to the significance of affirmation. Or, to put the same thing in different language, denial tends to become the equivalent of affirmation until at the highest point it has the same value. It seems to me, however, that these logicians state a truth in language that is misleading. In order to say anything intelligent about the value of denial we must first distinguish between its function and its implications. The function of denial is always and invariably removal. As such it is as unique in its character as affirmation. We have also seen that in its relation to the knowing process it is a principle of selection. In this regard it is also unique, and not to be merged in affirmation. But the implication of denial will in most instances, at least, be something positive. It will at least limit and define the sphere of alternatives by removing the false and incompatible. And as knowledge becomes richer and more specific a denial will come to point with greater and greater precision to an affirmation which will be involved in it by some relation of opposition. It is incident on the growth of knowledge that the system which it immanates becomes more closely knitted together and that the judgment functions become more specific. If knowledge could once complete itself we would then have a subject whose every affirmation would exclude a specific negation and whose every denial would lead by direct implication to a specific opposite affirmation.

THE PSYCHOLOGY OF SUFFICIENT REASON.

BY DR. W. M. URBAN.

Reader Princeton University.

§ 1. Among those who make earnest with the idea of genetic psychology it seems to be taken for granted that in some sense the relation between utility and knowledge is a close one—that the extension of the doctrine of Selection into the sphere of knowledge processes, whether as natural selection or selection of a peculiar sort, is warranted. That there is wide difference of opinion, however, as to the nature of that selection and of the accommodations that result, a moment's glance at the literature will show. The uncertainty and differences in the answer to this problem arise mostly from the natural difficulty of keeping the philosophical and psychological sides distinct, in which direction Spencer set an unfortunate example. It seems to be equally unwarranted, however, to consider the question definitely settled either positively or negatively by a one-sided consideration from the point of view either of psychology or of a theory of knowledge. The following paper has therefore nothing more in mind than a consideration of some psychological phenomena which point to a process of selection according to the principle of utility in the sphere of the higher knowledge processes.

§ 2. Genetic psychologists prefer to designate the adaptation of consciousness to its environment by means of intelligence as 'sufficient,' rather than 'necessary,' as in the case of lower psychic organisms. By that distinction they mean to indicate the element of 'subjectivity' which distinguishes the selection in the case of higher will acts from the outer necessity which controls the lower instinctive reactions. Thus Spencer makes a distinction between the 'necessity' of the organized reactions of instinct and the 'sufficiency' of the less stable rational reactions

growing out of the correspondence of ideas to external reality.¹ So also Professor Baldwin: "The principle of sufficient reason is subject to a corresponding genetic expression on the side of accommodation. Sufficient reason in the child's mind is an attitude, a belief, anything in its experience which tends to modify the course of its habitual reactions in a way that it must accept, endorse, believe. This has its sufficient reason, and he must accommodate to it."² With a consideration of the nature of subjective sufficiency is included, therefore, the elements for the solution of the problem of accommodation among intellectual processes.

§ 3. Sufficiency in the sphere of intelligent processes does indeed include much more complex elements than the simple necessity of reflex movement. If the hypothesis of a positive selective factor, over and above the negative function of natural selection, is necessary, as it seems to be, even for the explanation of accommodation in the sphere of reflexes, still more is this positive factor, in much more developed form, a primary requirement in the higher spheres. For, though both are alike in that they are reactions upon environment, they differ materially in the nature of that reaction.

In reflex movement there are two terms, the stimulus and the reaction, between which at least the scientific criterion of *likeness* of cause and effect may be found. They are both objective terms and experience tends to prove the constancy of the relation of stimulus to reaction on the pleasure-pain hypothesis. The higher apperceptive functions, on the contrary, have three terms, the stimulus, the supervening ideal and emotional complex which gathers about the stimulus, and the motor reaction which follows in the will act. Here an entirely new relation meets the eye. Instead of the relative constancy of the relation between stimulus and reaction, instead of the relative constancy of outer conditions, appears a practically absolute inconstancy. The number of possible complexes of ideas and emotions that gather about the stimulus is, to all intents and purposes, infinite. For the stimulus does not work directly as outer reality; but in

¹ Spencer, '*Principles of Psychology*,' Vol. I., Chap. 7.

² Baldwin, *Mental Development*, p. 323.

its place enters the complex 'motive,' which, though it stands in the place of outer reality, does not necessarily correspond to it, but oftener does not. The pleasure-pain hypothesis is not directly applicable, for the reason that pleasure and pain do not enter necessarily into these complexes, but are oftener merely suggested.

§ 4. We may, therefore, express the relation (*a*) between the 'motives' and the will act, or (*b*) between the subjective ground of a judgment and the judgment itself as sufficient reason, but not as necessary cause as in the relation of stimulus to reflex movement. This infinite variability of motives which allows us to speak of them as subjectively 'sufficient' but not as causally necessary is evident if we consider with what difficulty 'motives' objectively necessary are found for the simplest will acts.

The consequence of this uncertainty is that we confine ourselves to simple primal effects such as love, hate, etc., which we have, in a manner, objectified as real forces, or at best we make hypotheses on the analogy of our own experience. The personal equation of sufficiency is further observable in spheres not directly connected with the will—in the æsthetic and intellectual judgment. In all thought products the sufficiency lies not in the logical texture, but in the ethical and æsthetic feeling sources of the production. Almost every bit of original thought, especially where it is of the genius rank, must suffer the elimination by critical thought of just those subjective elements in which for the thinkers the sufficiency lay. The same is true in the reaction of the individual upon race beliefs and customs, speech, etc.; the personal equation is always the source of the sufficiency which determines his reaction. 'Characterologie' is, however, notably the despair of empirical science simply because of this law of infinite variability. To be sure, it has been sought to construct a psychology of metaphysical systems, but scarcely with success, even in the case of the non-school philosophers who carry their hearts on their sleeves. The important point is that if the law of selective accommodation is carried up into the sphere of intellectual functions, as a principle of explanation for the existence of our knowledge, the problem becomes extremely complex, because (*a*), as has been shown, the

reaction is no longer upon simple reality, but upon an intervening *motive* complex which shows infinite variations from reality, and (b) as a consequence of this infinite variability, instead of the law of simple 'autogeneity' of ends in instinctive reactions, we have the law of heterogeneity of ends as the governing principle of the higher psychological processes.

§ 5. If it were asked what in the nature of our psychological organism gives rise to this divergence of the motive, which takes the place of the stimulus, from the known reality from which the simple stimulus arises, the answer would come from almost every reader, the presence of the imaginative processes. To these is due the presence of such a law as that of the infinite heterogeneity of ends. If the simple stimulus, unmodified by imagination, was reacted upon, the conditions could be comparatively constant as in instinctive reactions. By imagination is meant, of course, not the vulgar conception of the phantasy which confines it to the sphere of the æsthetical shine nor of the narrow view of some psychologists which restricts it to a particular kind of apperceptive processes, but rather is it a term for that general *element in all apperceptive processes of a complete nature which selectively projects ideas before consciousness* in an emotional unity and sufficiency more complete than that of the merely associational relations. This conception is in full accord with the doctrine of Wundt which describes all those unitary complexes of ideas and feelings (*Gesamtvorstellungen*) which precede either judgments or will acts as the products of 'Phantasie-Thätigkeit' and its 'schöpferische Synthese' which he will have recognized as a thoroughgoing principle of all psychological processes.¹ That this general element of imagination is the source of the divergence of the motives as ideal content from reality is clear from the nature of these processes, by means of which our stimulus may bring about an infinite variety of imaginative complexes dependent upon the nature of the psychological organism.

§ 6. But it is exactly this characteristic of the imaginative processes which suggests them as a possible basis for a doctrine of accommodation. It is true that in imagination we see the

¹ *Grundriss der Psychologie* (1895) p. 367.

source of the divergence of motives from the real environment for which they stand; but in this very divergence is likewise seen the possibility of new adaptation, for this law of the heterogeneity of ends which has its root in imagination offers at least the material for new selection, if only there exists a principle of selection adequate to the demands made upon it. For this principle we need not look beyond the imaginative processes themselves; in their activity lies also a principle of selection which counteracts that element in imagination which works as a source of estrangement from the outer environment, or, if the expression be allowed, uses it as an element in a higher synthesis. The imaginative processes stand in marked contrast to the associations from which they rise in two particularly noticeable characteristics.

a. While the associations pass in succession, according to immanent causal laws, the imaginative processes are governed by a law relatively superior to the associational flow of ideas, by an immanent teleological principle, which, although it expresses itself in the already mentioned law of heterogeneity of ends, yet is at bottom ruled by one motive, namely, the reproduction of reality or the production of experiences analogous to reality. This '*Imaginatio*' is a struggle to reproduce reality by an imitation on the basis of the scattered feeling, and idea, memories which already exist in consciousness. The result of this is a feeling and ideal complex which possesses as its ground tone a 'reality feeling' very like to that of an actual experience.

b. As a consequence of its being governed by this motive, the process of imagination is marked by a certain wilfulness with which some associations are selected and others rejected, according to the criteria of this reality feeling. With this wilfulness comes a certain increase of motor energy, an excess which tends to express itself in actual will acts.

§ 7. A little reflection will suffice to show that these imaginative processes, thus described, are splendid attempts at association in a complete sphere of manifold association. These associations in their mechanical state, if not organized in the form of instinct, stand rather as a barrier to direct reflex accommodation to environment. They must first be brought into a

unitary complex of feelings and ideas, which shall at least relatively reflect the reality which comes to consciousness in the form of stimulus. The ruling criterion is the feeling of reality with which the imaginative complex, this imitation of reality, is clothed. This sense of reality, or 'sufficiency,' it is evident, belongs alone to the feeling side of the complex, for the necessary relations of the ideas come to light first through reflection upon the results of the process, either in the judgment or in the will act, and its relation of advantage and disadvantage in the environment. Until the judgment or will act actually takes place and is reflected upon as a part of objective knowledge or of actual objective reality, that is *retrospectively*, it appeals to consciousness only as subjectively sufficient. For the sense of reality which attaches to the imaginative processes, as background to the judgment or act, arises from the fact that there has been reproduced in consciousness the same organic state (or at least with only slight modification) as existed at an earlier time when reality was directly reacted upon. This means, of course, that the same general affect tone, together with the particular feelings of that experience, have been reproduced by a new stimulus, and consequently that stimulus, by reason of the emotional complex gathered about it, is sufficient to bring about the habitual reaction or one nearly like it.

From these considerations arises a distinction which is fundamental to the whole problem of genetic psychology, namely, the difference between the motor side, which has its source in the feelings, and the immanent relations among the ideas; a distinction which is to be made in every psychological process, especially in the imaginative processes. Both the idea and the motor expression are parallel results of the one psychological process, but stand in no relation of cause and effect. The ideas are not motives to the will act, much less are they causes of the affect side of the process, but both are results of a common, more primal process of imagination.

§ 8. With this distinction, between the 'affect' or force side of the process and the ideal complex, we have a principle by means of which we may more clearly understand the motor expressions which result upon the imaginative processes. When

once the imaginative intuition of reality, with its affect of 'sufficiency' and reality, has come into existence under the influence of the motive of accommodation to the stimulus, the 'motor excess' of this process may express itself in either of two ways. Either the stimulus upon which the imagination followed appeals so directly to the pleasure and pain feelings, or the reality feeling is of such intensity that a will act follows as its expression, or else these conditions do not exist and the motor excess is turned upon the ideal content in a series of apperceptive analytical processes which determine the relations of the ideas among themselves. In the first case the 'force' of the process has found vent in a will act which brings the organism into direct relation to outer reality, in the form of accommodation; in the latter this natural expression has been retarded or prevented, and the energy is expended upon an analysis of the ideal complex, where the theoretical relation of the ideas to each other becomes the problem. The important point is that both of these widely different results spring out of the common primal term—the Imaginative Processes. Out of the union of ideas and emotional elements which takes place under the motive of the imitation of reality, the 'sufficiency' of both the will act and the judgment arises. The 'sufficiency' lies, in both cases, in the affect side of the complex; the coming into prominence of either the motor expression in the will act, or of the theoretical judgment upon the relations of the ideas, is dependent upon laws which we have now to consider. For just here lies the problem of Selection; if like imaginative processes which work under the teleological norm of an imitation of reality at one time pass over into motor accommodation to environment and again fall back upon their own ideal content, on what principle is the selection made as to which complex shall result in will act and which shall not?

§ 9. Here, it would seem, is the place to call in the simple principle of utility, and properly understood, it seems to us to be the solution of the problem. The subjective 'sufficiency' of the motives of will acts and of the 'grounds' of judgments alike was seen to lie in the affective side of the imaginative processes which precede them. The characteristic of this

affect is that it is a strong sense of reality, made up of the memory feelings of prior experiences. All of these complexes have the feeling of reality, closely related to the reality of perception in some degree, but not all have the affect side predominant, in the sense that it appeals directly to the fundamental feelings of pleasure and pain, as a direct stimulus, and therefore not all are brought directly into relations to the principle of utility. In the place of the more definite sense of utility or disadvantage which attaches to the 'motives,' or the imaginative processes which result in motor reactions upon environment, in those complexes which result in judgments upon the ideas, the concept of general worth or value must be substituted. That is, the reality feeling of the imaginative complex is of such a nature that it is handled as of value or worth to consciousness, but not as so intense as to bring forth a will reaction—that is it does not involve a suggestion of immediate pain or pleasure to the organism.

§ 10. The problem of Selective accommodation may then be stated as follows: How is it possible that from motor reactions, which are based entirely upon their utility to the organism—that is, will acts of accommodation to environment—imaginative complexes may arise which have only the predicate 'worth;' that is, which result not in immediate reaction upon environment, but in judgments as to the relations of the ideas? How, in other words, is the abstract concept of *truth* to be connected with the concrete utility of the particular experience.

The answer to this is to be found in the nature of the imitative process of Imagination. The primary type of this process is that in which the affect side prevails and the consequent motor reaction follows. As a matter of fact, all observations tend to show that the less developed the psychological organism the greater the number of completed will acts in proportion to those which are not allowed to follow their course. The more developed the psychological state the greater the degree of selection manifested in the will acts, that is, the less the emotional complexes are allowed to have their natural motor discharge. It follows that we must look upon all imaginative processes as originally ending in will acts; only gradually did

imaginative complexes arise in which the attention was turned upon the ideal complex which gathers about the stimulus, instead of the stimulus itself.

§ 11. Definitely formulated then, a theory of selection which adjusted itself to these facts would read somewhat as follows: Reaction of the organism to its environment in the sphere of intelligence does not take place directly upon the stimulus, but through the mediation of ideal complexes which stand for the external reality. These complexes are of the nature of imitations of external reality in that they are the result of imaginative processes which gather together the experience of the past under the teleological criterion of reproduction of the reality feelings of the past. All of the infinite number of complexes thus possible tend to go over into motor expressions in will acts, that is, in accommodation to environment. Some of these are favorable, that is, the imaginative complexes correspond to reality, and some are not favorable, have not corresponded to actual reality. Gradually the number of imaginative complexes which go over into will acts becomes proportionately smaller by means of this selection, and the number of those which are prevented because they have proved themselves not to be in harmony with the external reality, the reaction upon them having failed to be accommodative, becomes proportionately larger.

Thus arises gradually a sphere of imaginative processes which express this motor energy only in appreciative analytical acts upon themselves in the manner previously described. These relations thus developed are of general worth or truth instead of immediate practical advantage or disadvantage.

The nature of the selection becomes clearer from the consideration of certain pathological cases. Hallucination and illusion are conditions where, or account of hyperæsthesia, imaginative processes retain their reality feeling, although repeated motor reactions upon them fail to be accommodative. The immediate reality feeling, growing out of the intensity of the emotion is so strong that the disadvantages (often the *pain*), of reaction upon the external world fail to modify or destroy the imaginative complex. The normal imaginative complex is, however, subject to modification from the feelings which arise

as the result of the reaction. And herein lies the possibility of new accommodations.

§ 12. But how, it will be asked, can such a theory of selection account for the logical and *a priori* relations among the ideas which tend more and more to segregate themselves from the direct accommodations. Surely they are not the products of selective accommodation and yet an extension of the principle of selection to the sphere of the intellectual processes, must be on the basis of the principle: that only *those ideas are true which have proven to be of utility*. A little reflection will suffice to discover a fallacy in this principle. Ideas are never of utility; only feelings and states which are consequent upon accommodations are of utility. Ideas are only signs for psychological states. To speak of ideas as being of utility implies a point of view which overrides the boundaries of psychology, and falls into the fatal error of Spencer, of basing the whole of genetic psychology on the metaphysical hypothesis of a correspondence between the ideas and reality. This distinction between the 'force' side and the 'ideal' side of the imitative processes, which is expressed in the sentence "the idea does not work but only the process of getting the idea," enables us to separate completely the dynamical and utility side of psychological processes from the logical relations of the ideal content that results. And this is an absolutely necessary presupposition of any genetic study. The fundamental laws of the ideal side of our conscious complexes are laws of relations based upon the analytical criteria of 'clearness and distinctness.' They belong distinctly to the peculiar sphere of ideas and have nothing to do with the problem of organic accommodation. In the latter sphere the criteria, as we have already seen, are distinctly affective, growing out of the feeling of reality and the pleasure and pain which accompany it. The ideal relations as such lie, accordingly, entirely outside the line of direct accommodations. They work only indirectly in future accommodations, in that when consciousness is gathered together again in a new imaginative complex for a new motor reaction, the ideal content appears in more distinct and perhaps modified relations, but again the 'sufficiency' and the accommodation will lie in the affect side.

§ 13. But is not the fallacy in the preceding expression that "only those *ideas* are true which have proven themselves to be of utility" the stumbling block to any application of genetic selection in the intellectual sphere; a final barrier to any connection between utility-selection and truth? Were it not better to say: *our ideas must be true, that is correspond to outer reality, if the acts based upon them are to be advantageous?* Here the correspondence between our ideas and outer reality is assumed and the utility of our acts concluded from the assumption. The primacy of immanent *a priori* relations among ideas is taken for granted as the source of a necessary accommodation to an environment corresponding to these ideas. On the contrary, it could be claimed that ideas must prove themselves useful, before they can obtain a permanent place in the content of our consciousness, they must be seen by actual practice to correspond to reality before they can be distinguished as permanent truth from the mere fictions of the imagination. This apparent antinomy which so often stands in the way of reconciliation of empirical and *a priori* theories of knowledge rests upon different ways of looking at a single process or fact. In the first part of the antinomy is expressed an objective attitude toward accommodations after they have actually taken place. We conclude from a favorable accommodation on the part of a particular psychological organism as a consequence, to a knowledge of the true relations of things in this consciousness as ground. On the other hand, if we say that the ideas must be of utility to be true, we conclude from the subjective ground to an objective consequence, because from our standpoint, as practical agents, it is alone those ideas which appeal to us as of worth which correspond to this practical accommodation which we have made in will act.

§ 14. This difference in attitude corresponds to a distinction which can be made in the general body of truth. The relations among individual elements of scientific truth are true in a sense that the whole of truth is not, for they are analytically determinable according to the logical criteria immanent in the ideas themselves. The whole truth, however, has no such criteria as Descartes clearly saw when he made the whole of the truth de-

pendent upon the certainty of the intuition of the self, that is upon a psychological term of belief. The self cannot be doubted because there are no higher criteria according to which it can be proved. The reality feeling of the self is, therefore, the criterion of the truth of all the content in the consciousness of the self. So also in this case the relation may be expressed epigrammatically in the sentence: *The whole of truth rests upon utility which goes back to the psychological affective side, its parts, however, upon analytical and logical necessity.* This contradiction finds its psychological solution, and that is all that concerns us, in the reduction of both terms to a more primal term, the imaginative processes. These are found to be the background of will acts and judgments alike. The 'sufficiency' of the 'motive' as well as that of the psychological ground of a judgment lies in each case in the affective side of the imaginative complex. Of these two possible results of the imaginative processes, the will expression is the more primal. The relation of the practical will side of consciousness to reality is closer and more fundamental than that of the ideal. In its accommodation, therefore, is to be found the source of all new content in consciousness. The reflective processes which are the result of the turning of the motor force or attention upon the ideal content are the secondary results when the natural reaction is hindered or retarded. Thus arises gradually a sphere of segregated truth, which is first of all of theoretical and general worth, and only indirectly of practical utility. The individual acts of will which are based upon the utility to the organism whose reactions upon environment they are, must tend in the long run to fix the results as necessary for the race. When, however, these results are so recognized, they become parts of a settled and independent body of truth, which has its own laws outside the sphere of the utility reactions which first brought it into being.

§ 15. A study of the development of child consciousness and of primitive peoples would present a mass of material which tends to prove that intelligent accommodation to environment, proceeds upon the principle of a selective *reduction* of imaginative reactions upon given kinds of environment to permanent

bounds. That is in the proportion that *extension* of the possibilities open for the imagination is reduced, in equal proportion, is the *intension* increased. In the young child or in the primitive man the imagination clothes elements of environment of the most divergent nature with the same attributes, mostly personal, and reacts upon them accordingly, or again the same stimulus is at different times reacted upon with different imagination content, simply because the reality feeling does not work definitely and certain. Thus arise the phenomena of superstition—the freedom from which is a continuous process of accommodation to environment, and which, when completed, may leave behind a new science as illustrated in the development of chemistry from alchemy. When such a stage is reached where a definite amount of theoretical material is segregated by the selective reduction of the number of the possible reactions or imaginations, the imaginative processes, though restricted in extension to this material, grow in intension, and the process is then continued in the form of scientific hypothesis. But all this leads us into the sphere of comparative psychology, while our only object was an analysis of the psychological processes which point to a doctrine of selective accommodation.

In closing, the interesting fact may be noted that both Kant and Herbart find the subjective sufficiency of judgments to lie in the imaginative processes. Kant, in his subjective deduction of the categories finds in the transcendental synthesis of imagination the ground of the union of the sense intuition and the logical forms. Herbart likewise finds the psychological grounds of sufficient reason in the imagination. With both, however, the imagination is at bottom a metaphysical term, and, consequently, though both gave valuable suggestions as to the nature of the psychological grounds of judgments, it is only suggestively that their doctrines of imagination can be referred to in this connection. The above developed principle of selective accommodation rests alone on the analysis of the psychological processes called imagination.

DETERMINATE EVOLUTION.¹

BY J. MARK BALDWIN.

I. ORGANIC SELECTION.

Admitting the possible truth of either of the current doctrines of heredity, yet there are certain defects inherent in both of them. Natural Selection, considered merely as a principle of survival, is admitted by all. It fails, however, (1) to account for the lines of progress shown in evolution where the variations supposed to have been selected were not of importance enough at first to keep alive the creatures having them (*i. e.*, were not of 'selective value'). The examination of series of fossil remains, by the paleontologists, shows structures arising with very small and insignificant beginnings.² Further, (2) in cases where correlations of structures and functions are in question, as in the case of complex animal instincts, it is difficult to see what utility the partial correlations could have had which would necessarily precede the full rise of the instinct; and yet it is impossible to believe that these correlations could have arisen by the law of variation all at once as complete functions.³ These two great objections to the 'adequacy of natural selection' are so impressive that the Neo-Darwinians have felt obliged to deal with them. The first objection may be called that from 'determinate evolution,' and the latter that from 'correlated variations.'

On the other hand the doctrine of use-inheritance or Lamarckism is open to equally grave difficulties in my opinion. (1) It is a pure assumption that any such inheritance takes place. The direct evidence is practically nothing.⁴ No unequivocal case of the inheritance of the normal effects of use or disuse has yet been cited. Again (2) it proves too much, seeing that if it actually operated as a general principle it would hinder rather than advance evolution in its higher reaches. For, first, in the more variable functions of life it would produce conflicting lines of inheritance of every degree of advantage and disadvantage, and these would very largely neutralize one another, giving a sort of functional 'panmixia' of inherited habits analogous to the panmixia of variations which arises when natural selection is not operative. Again, in cases in which the functions or acquired habits are so widespread

¹ Matter added in the foreign editions of the author's 'Mental Development in the Child and the Race.'

² Cf. the statement of this objection by Osborn, *Amer. Naturalist*, March, 1891.

³ Cf. Romanes, *Darwin and after Darwin*, II., chap. 3.

⁴ See the candid statement of Romanes, *loc. cit.*; and Morgan, *Habit and Instinct*, Chap. XIII.

and constant as to produce similar 'set' habits in the individuals, the inheritance of these habits would produce, in a relatively constant environment, such a stereotyped series of functions, of the instinctive type, that the plasticity necessary to the acquirement of new functions to any great extent would be destroyed. This type of evolution is seen in the case of certain insects which live by complex instincts; and however these instincts may have been acquired, they may yet be cited to show the sort of creatures which the free operation of use-inheritance would produce. Yet just this state of things would again militate against continued use-inheritance, as a general principle of evolution; for as instinct increases, ability to learn decreases, and so each generation would have less acquisition to hand on by heredity. So use-inheritance would very soon run itself out. Further, (3) the main criticism of the principle of natural selection cited above from the paleontologists, *i. e.*, that from 'determinate evolution,' is not met by use-inheritance; since the determinate lines of evolution are frequently, as in the case of teeth and bony structures, in characters which in the early stages of their appearance are not modified in the direction in question, during the lifetime of the creatures which have them. And, finally, (4) if it can be shown that natural selection, which all admit to be in operation in any case, can be supplemented by any principle which will meet these objections better than that of use-inheritance, then such a principle may be considered in some degree a direct substitute for the Lamarckian factor.

There is another influence at work, I think, which is directly supplementary to natural selection, *i. e.*, *Organic Selection*.

Put very generally, this principle may be stated as follows: acquired characters, or modifications, or individual adaptations—all that we are familiar with in the earlier chapters under the term *Accommodations*—while not directly inherited, are yet influential in determining the course of evolution indirectly. For such modifications and accommodations keep certain animals alive, in this way screen the variations which they represent from the action of natural selection, and so allow new variations in the same directions to arise in the next and following generations; while variations in other directions are not thus kept alive and so are lost. The species will therefore make progress in the same directions as those first marked out by the acquired modifications, and will gradually 'pick up,' by congenital variation, the same characters which were at first only individually acquired. The result will be the same, as to these characters, as if they had been directly inherited, and the appearance of such heredity in these cases,

at least, will be fully explained. While the long continued operation of the principle will account for 'determinate' lines of change.

This principle comes to mediate to a considerable degree between the two rival theories, since it goes far to meet the objections to both of them. In the first place, the two great objections as stated above to the ordinary Natural Selection theory are met by it. (1) The 'determinate' direction in the evolution is secured by the indirect directive influence of Organic Selection, in all cases in which the direction which phylogenetic evolution takes is the same as that which was taken by individual modifications in earlier generations. For where the variations in the early stages of the character in question were not of selective value, there we may suppose the individual accommodations have supplemented them and so kept them in existence. An instance is seen in the fact that young chicks and ducks which have no instinct to take up water when they see it,¹ and would perish if dependent upon the congenital variations which they have, nevertheless imitate the mother fowl, and, thus by supplementing their congenital equipment, are so kept alive. In other fowls the drinking instinct has gone on to perfection and become self-acting. Here the accommodation secured by imitation saves the species—apart from their getting water at first accidentally—and directs its future development. Farther (2) in cases of 'correlated variations'—the second objection urged above to the exclusive operation of Natural Selection—the same influence of Organic Selection is seen. For the variations which are not adequate at first, or are only partially correlated, are supplemented by the adaptations which the creature makes, and so the species has the time to perfect its inadequate congenital mechanism. On this hypothesis it is no longer an objection to the general origin of complex instincts without use-inheritance, that these complex correlations could not have come into existence all at once; since this principle gives the species time to accumulate and perfect its organization of them.

Similarly, the objections cited above to the theory of use-inheritance can not be brought against Organic Selection. In the first place (1) the more trivial and varied experiences of individuals—such as bodily mutilations, etc.—which it is not desirable to inherit, whether good or bad in themselves, would not be perpetuated in the development of the race, since organic selection would set a premium only on the variations which were important enough to be of some material use or others which were correlated with them. These being of

¹ See Morgan, *Habit and Instinct*, pp. 44 f. and his citations from Eimer, Spalding, and Mills.

such importance, the species would accumulate the variations necessary to them, and the individuals would be relieved of the necessity of making the private adaptations over again in each generation. Again (2) there would be no tendency to the exclusive production of reflexes, as would be the case under use-inheritance; since in cases in which the continued accomplishment of a function by individual accommodation was of greater utility than its accomplishment by reflexes or instinct—in these cases the former way will be perpetuated by natural selection. In the case of intelligent adaptations, for example, the increase of the intelligence with the nervous plasticity which it requires is of the greatest importance; we find that creatures having intelligence continue to acquire their adaptations intelligently with the minimum of instinctive equipment.¹ There is thus a constant interplay between instinct and accommodation, as the emergencies of the environment require the survival of one type of function or the other. This is illustrated by the fact that in creatures of intelligence we find sometimes both the instinctive and also the intelligent performance of the same function; each serving a separate utility.²

(3). The remaining objection—and it holds equally of both the current views—is that arising from the cases of structures which begin in a very small way with no apparent utility, such as the bony protuberances in places where horns afterwards develop, and in certain small changes in the evolution of mammalian teeth; which afterwards progress regularly from one generation to another until they become of some utility. While it is not clear that Organic Selection completely accounts for these cases, yet it is quite possible that it aids us in the matter; for the assumption is admissible that in their small beginnings these characters were correlated with useful functions or variations, and that the Organic or Natural Selection of the latter in a progressive way has secured the accumulation of these characters also. The facts of correlation are so little known, while yet the correlation itself is so universal, that no dogmatism is justified on either side; the less, perhaps on the side of the paleontologists who assert that these cases can not be explained by Natural Selection even when supplemented by Organic Selection; for when we enquire into the state of the evidence for the so-called ‘determinate variations’ which are supposed in these cases, we find that it is very precarious.³

¹ Groos (*Die Spiele der Thiere*, p. 65 f.) has pointed out the function of imitation as aiding the growth of intelligence with the breaking up of instincts under the operation of natural selection.

² Baldwin, *Science*, Apl. 10, 1896.

³ For example, the only way to establish ‘determinate variations’ would

We come to the view, therefore, that evolution from generation to generation has probably proceeded by the operation of Natural Selection upon variations with the assistance of the Organic Selection of co-incident¹ variations (*i. e.*, those which reproduce congenitally the acquisitions of the individuals). And we derive a view of the relation of ontogeny to phylogeny all through the animal series. All the influences which work to assist the animal to make adaptations or accommodations will unite to give directive determination to the course of evolution. These influences we may call 'orthoplastic' or directive influences. And the general fact that evolution has a directive determination through organic selection we may call 'Orthoplasmy.'²

As to detailed evidence of the action of Organic Selection, this is not the place to present it. It is well-nigh coextensive however with that for Natural Selection; for the cases where natural selection operates to preserve creatures because they adapt themselves to their environment are everywhere to be seen, and in all such cases Organic Selection is operative. Positive evidence in the shape of cases is however to be found in the papers of the writer and others on the subject.³

be to examine all the individuals of a given generation in respect to a given quality, and compare their mean with *the mean of their parents—not with the mean of all the individuals of the earlier generation*. For some influence, such as Organic Selection, might have preserved only a remnant of the earlier generation, and in this way the mean of the variations of the following generation may be shifted and give the appearance of being determinate, while the variations themselves remain indeterminate. And again, the paleontologists have no means of saying how old one of these fossil creatures had to be in order to develop the character in question. It may be that a certain age was necessary and that the variations which he finds lacking would have existed if their possessors had not fallen by natural selection before they were old enough to develop this character and deposit it with their bones.

¹A term suggested by Professor Lloyd Morgan.

²These terms are akin to 'orthogenic' and 'orthogenesis' used by Eimer (*Verh. der Deutsch. Zool. Gesell.*, 1895); his terms are not adopted by me however, for the exact meaning given above, since Eimer's view directly implicates use-inheritance and 'determinate variations' which are here rejected. On the use of these and other terms see *Science*, Apl. 23, and *Nature*, Apl. 15, 1897.

³It may be in place to recall something of the history of this suggestion as to Organic Selection and cite some of the publications bearing upon it. The present writer indicated it (only) in the first edition of this work (Feb. 1895), presented it fully with especial reference to the origin of instinct in *Science*, March 20, 1896, and developed it in many of its applications in an article entitled 'A New Factor in Evolution,' *American Naturalist*, June and July, 1896 (reprinted in *Princeton Contrib. to Psychology*, I., 4, September, 1896). Professor H. F. Osborn expressed similar views briefly in an abstract in *Science*, April 3, 1896, p. 530; and more fully in *Science*, November 27, 1896.

II. THE DIRECTIVE FACTOR.

We have now seen some reason for the reproduction of individual or ontogenetic accommodations in race progress. The truth of Organic Selection is quite distinct, of course, from the truth of any particular doctrine as to how the accommodations in the life of the individual are effected; it may be that there are as many ways of doing this as the usual language of daily life implies, *i. e.*, mechanical, nervous, intelligent, etc.

Yet when we come to weigh the conclusions to which our earlier discussions have brought us, and remember that the type of reaction, which is everywhere present in the individual's accommodation, is the 'circular reaction' working by functional selection from over-produced movements, we see where a real orthoplastic influence in biological progress lies. The individuals accommodate by such functional selection from over-produced movements; this keeps them alive while others die; the variations which are represented in them are thus kept in existence, and further variations are allowed in the same direction. This goes on until the accumulated variations become independent of the process of individual accommodation, as congenital instincts. Thus are added to the acquisitions of the species the accommodations secured by the individuals. So race progress shows a series of adaptations which corresponds to the series of individual accommodations.

It may be remarked also that when the intelligence has reached considerable development, as in the case of man, it will outrank all other means of individual accommodation. In Intelligence and Will (as will appear below)¹ the circular form of reaction becomes highly developed, and the result then is that the intelligence and the social life which it makes possible so far control the acquisitions of life as greatly to limit the action of natural selection as a law of evolution. This may be merely indicated here; the additional note below will take the subject further in the treatment of what then becomes the means of transmission from generation to generation, a form of handing down which, in contrast with physical heredity, we may call 'Social Heredity.'

Professor C. Lloyd Morgan also printed similar views, *Science*, November 20, 1896, and in his book, *Habit and Instinct*, November, 1896. The essential position was reached independently by each of these writers and has been developed by correspondence since their first publication of it.

¹ *I. e.* in the volume, Chaps. X. to XIII.

III. INTELLIGENT DIRECTION AND SOCIAL PROGRESS.

The view of biological evolution already brought out has led us to the opinion that the accommodations secured by the individuals of a species are the determining factor in the progress which the species makes, since, although we can not hold that these accommodations, or the modifications which are effected by them, are directly inherited from father to son, nevertheless by the working of Organic Selection with the subsequent accumulation of coincident variations the course of biological evolution is directed in the channels first marked out by individual adaptations. The means of accommodation were called above orthoplastic influences in view of the directive trend which they give to the progress of the species.

It was also intimated, in the earlier section, that when the intelligence once comes to play an important part in the accommodations of the individuals, then we should expect that it would be the controlling factor in race-progress. This happens in two ways which may now allow of brief statement.

1. The intelligence represents the highest and most specialized form of accommodation by 'circular reaction.' With it goes, on the active side, the great fact of volition which springs directly out of the imitative instinct of the child. It therefore becomes the goal of organic fitness to secure the best intelligence. On the organic side, intelligence is correlated with plasticity in brain structure. Thinking and willing stand for the opposite of that fixity of structure and directness of reaction which characterize the life of instinct. Progress in intelligence, therefore, represents readiness for much acquisition, together with very little congenital instinctive equipment.

It is easy to see the effects of this. The intelligence secures the widest possible range of personal adaptations, and by so doing widens the sphere of Organic Selection, so that the creature which thinks has a general screen from the action of natural selection. The struggle for existence, depending upon the physical qualities which the animals rely on, is largely done away with.

This means that with the growth of intelligence, creatures free themselves more and more from Natural Selection. Variations of a physical kind come to have within limits an equal chance to survive. Progress then depends on the one kind of variation which represents improved intelligence—variations in brain structure with the organic correlations which favor them—more than on other kinds.

2. The other consideration tends in the same direction. With

the intelligence comes the growth of sentiment, especially the great class of Social Sentiments, and their outcome the ethical and religious sentiments. We have seen in earlier chapters how the sense of personality or self, which is the kernel of intelligent growth involves the social environment and reflects it. Now this social sense also acts wherever it exists, as an 'orthoplastic' influence—a directive influence, through Organic Selection, upon the course of evolution. In the animal world it is of importance enough to have been seized upon and made instinctive: animal association acts to screen certain groups of creatures from the operation of Natural Selection.

In man the social sentiment keeps pace with his intelligence, and so enables him again to discount natural selection by coöperation with his brethren. From childhood up the individual is screened from the physical evils of the world by his fellows. So another reason appears for considering the course of evolution to be now dominated by the intelligence.

But, it may be asked, does not this render progress impossible, seeing that it is only through the operation of Natural Selection upon variations—even allowing for Organic Selection—that progress depends? This may be answered in the affirmative, so far as progress by physical heredity is concerned. Not only do we not find such progress, but the researches of Galton, Weismann and others show that there is probably little or no progress, even in intelligence, from father to son. The great man who comes as a variation does not have sons as great as he. Intermarriage keeps the level of intelligent endowment at a relatively stable quantity, by what Galton has called 'regression.'

Yet there is progress of another kind. With intelligence comes educability. Each generation is educated in the acquisitions of earlier generations. There is in every community a greater or less mass of so-called 'Tradition' which is handed down with constant increments, from one generation to another. The young creature grows up into this tradition by the process of imitative absorption which has been called above 'Social Heredity.'¹ This directly takes the place of physical heredity as a means of transmission of many of the acquisitions which are at first the result of private intelligence, and tends to free the species from the dependence upon variations—except intellectual variations—just as the general growth of intelligence and sentiment tends to free it from the law of natural selection.

¹P. 361 and 364 (as in the first edition). See article on 'Consciousness and Evolution,' *Science*, August 23, 1895, reprinted with discussion by Prof. E. D. Cope and the writer in the *Amer. Naturalist*, Nos. from April to July, 1896.

These general truths can not be expanded here; they belong to the theory of social evolution. Yet they should be noted for certain reasons which are pertinent to our general topic, and which I may briefly mention.

First, it should be said that this progress in emancipation from the operation of natural selection and from dependence upon variations, is not limited to human life. It arises from the operation of the principle which has all the while given direction to organic evolution; the principle that individual accommodations set the direction of evolution, by what is called Organic Selection. It is only a widening of the sphere of accommodation in the way which is called intelligent, with its accompanying tendency to social life, that has produced the deflection of the stream which is so marked in human development. And as to the existence of 'Tradition' and 'Social Heredity' among animals, recent biological research and observations are emphasizing them both. Wallace and Hudson have pointed out the great importance of imitation in carrying on the habits of certain species; Weismann shows the importance of tradition as against Spencer's claim that mental gains are inherited; Lloyd Morgan has observed in great detail the action of social heredity in actually keeping young fowls alive and so allowing the perpetuation of the species, and Wesley Mills has shown the imperfection of instinct in many cases with the accompanying dependence of the creatures upon social, imitative and intelligent action.

Second, it gives a transition from animal to human organization, and from biological to social evolution, which does not involve a break in the chain of influences already present in all the development of life.

STUDIES FROM THE PRINCETON PSYCHOLOGICAL LABORATORY, VI-VII.

VI. THE REACTION TIME OF COUNTING.

BY PROFESSOR H. C. WARREN.

Princeton University.

I. INTRODUCTION.

The problem underlying this study was the question as to how we determine the number of things in a group. The mental process concerned in this determination is evidently not the same as the function technically known to experimental psychologists as *discrimination*. The latter consists in distinguishing between two or more different things; an object is ascertained, by means of certain marks or characteristics, to be the thing sought for and not something else; or the absence of these characteristics is noted and it is thus known not to be the thing sought for. It is also a mental process distinct from *recognition*; we speak (technically) of recognizing an object or objects when we recall their former presence in consciousness by means of certain marks and are thereby able to class them or give them a name. The *knowledge of the number of things in a group*, on the other hand, is independent of marks or differences. Number depends solely on the *distinctness* or *separateness* of the objects; it has nothing to do with their complexion. The word discrimination might readily be applied to the numbering process, and so might the word recognition; but if this were done it could only be through a change from their techni-

cal connotation; 'numbering' is very different from the processes to which these two words are applied by experimentalists; the mark of 'five-ness,' if we may use such a term, is simply the spatial or temporal distinctness of the objects in the group—any or all of the objects can be exchanged for any others, however different, and the 'five-ness' remains unaltered; this does not hold true in the case of ordinary recognition or discrimination.

It is not necessary here to enter into any discussion of the origin of the concepts 'one,' 'two,' 'three,' etc. This is an entirely separate question, which has already received considerable attention from psychologists and mathematicians.¹ In the present study we were concerned solely with the proper application of these terms to given groups of objects. That is, we were to investigate the concrete process of *numbering*, rather than the process of acquiring the abstract *number concepts*.

Whatever the nature of this numbering process, and whatever different kinds of numbering there may be, it is proper enough to denote the function by the term *counting*, as we shall do throughout this paper. But we must distinguish at the outset between several varieties of counting. The most important distinction is that between counting proper and inferential counting. In the former, objects are added up, so to speak, by a sort of mental 'one-two-three-ing;' in the latter, some clue is given by the form of the group, the amount of space it occupies, the amount of time required to survey it, etc.; thus, the familiar quincunx form (:::) is taken in as a whole—the form of the figure is associated with the number-name, by a mass of former experiences, as firmly as is the symbol '5.' The present study was concerned primarily with the former process; the latter is a species of association or inference (as the case may be), whose investigation involves a different problem; indeed, its chief rôle in our study was that of an enemy to be thwarted at all hazards.

¹ On this point see 'The Number Concept,' by L. L. Conant, New York, Macmillans, 1896, and 'The Number System of Algebra,' by H. B. Fine, Boston, Leach, Shewell & Sanborn, 1897.

A further distinction is to be noted, within the process of counting proper, between that which is practically instantaneous and that which involves the expenditure of time. If it takes no more time to count Three¹ than to count Two or One, it is evident that the apprehension of each separate object does not involve time; if the reaction time of these numbers be practically the same, then their counting proceeds by an apprehension of the group as a whole, rather than by successive apprehension of its members. Whereas, if the reaction time of Four (say) is longer, the increment is time consumed in apprehending the extra unit. We may call these two processes *perceptive* and *progressive* counting, respectively; add to this the process already noted, *inferential* counting, and we have three distinct methods of counting. I give this classification here without discussing its practical bearing (which will appear later), in order to make clear the nature of the problem and the precautions which had to be taken in the investigation to avoid confusion between the various distinct processes.

On the basis of this division two problems appeared which it was the object of this study to investigate. These were: (1) What is the largest number that can be counted by a single act of apprehension—on the one hand, without expenditure of extra time in taking in each additional object; on the other, without the assistance of association or inference? This is the problem of the limit of perceptive counting. (2) What is the part played by association and inference in our habitual acts of counting?

A third problem might have been added, viz., as to the law by which the time of progressive counting increases with the increase of number—in other words, the rate of progressive counting. This last inquiry was not followed up on account of its great complexity: it would have required a large amount of time to carry out the experiments, and the problem itself presented difficulties, on account of certain disturbing factors entering in, *e. g.*, the eye movements necessary to take in any extensive group of objects. As between the other two problems, the present investigation was more particularly concerned with the first.

¹ To avoid confusion the number-names will be printed with a capital.

II. HISTORICAL.

I may point out, first of all, the close relation that exists between this problem and that of the so-called *area of consciousness*. The area of consciousness (Umfang des Bewusstseins), as understood by the Leipzig investigators, is the sum total of impressions that can be held in consciousness at one time. The classic experiments of Dietze¹ on this topic aimed to determine this sum for a single case (the simplest) by means of groups of successive sounds. The subject was forbidden to count the sounds—he was to determine the difference between two groups after both had been given, by the mere fact of retaining all the members of each group in consciousness at once. The groups were compared as equal, greater or smaller, the hypothesis being that as long as this could be done correctly the subject must have had a simultaneous impression of each entire group. Dietze's subjects were able to distinguish differences correctly up to Sixteen when the sounds were uniform, and up to as many as Forty when each group was divided into subgroups of Eight by rhythmic accentuation. The highest numbers in each case were reached only when the rate of succession of the sounds was most favorable; thus these numbers, if the hypothesis be correct, represent the very maximum area of consciousness. The area of consciousness in the case of counting is a somewhat different thing. In Dietze's problem no mental act was involved *during the experiment* but the retention of the sounds in consciousness as distinct; in counting an active effort is required to bring the units together under the form of a number-concept. Still, I am of the opinion that the two problems really belong to the same category, the difference consisting chiefly in the presence of an act of apperception in counting, while Dietze's experiments involved merely perception. My subjects were not able to *gather in* at once numbers nearly so large as Dietze's could *hold together*; this was to be expected to some extent; but the wide difference between the two results, which will appear later, leads me to question whether Dietze's subjects succeeded altogether in avoiding counting (*i. e.*, progressive counting), and still more whether they

¹ Philos. Stud., 1885, II., 362 ff.

did not rely somewhat upon the length of time, and *infer* the size of the group from this—a tendency which (in another form) I found exceedingly difficult to prevent among my own subjects. In view of the importance and fundamental character of this problem, it seems strange that no one has ever undertaken to repeat Dietze's experiments.

Another problem somewhat analogous to the present one is the number of objects, letters, etc., that can be recognized at the same time. An investigation of this subject was made by Cattell¹ at Leipzig, in connection with his reaction time experiments, by a method of combined simultaneous and successive exposure. The objects were passed across a slit in a screen, the slit being varied in size so that any desired number of the objects could be seen simultaneously. He found that three, four or five letters could be recognized when passing at once—the maximum differing within these limits for different subjects; this was apart from the grouping of the letters into words, which, of course, involves association and is a very different process from the one under investigation.

The problem of counting was taken up by Cattell in a later investigation,² where he places it under the head of area of consciousness. Cattell's experiments consisted in exposing to view simultaneously and for a very short period (10σ) a number of lines drawn on cardboard; the subject was required to determine the number of lines on the card; the apparatus employed was a falling screen. In these experiments the method of right and wrong cases was used. The largest number for which the right answers exceeded the wrong varied between Five and Eleven, according to the subject. The higher numbers, however, were only correctly counted by those who had made many trials; this leads to the suggestion that the subject may have become familiar with the number of lines on each card in the course of his practice, and that he may have afterwards judged the number from the width of space occupied by the lines on the card—an inferential process again. On this account Cattell's results seem open to question,

¹Philos. Stud., 1885, II., 635 ff.

²Philos. Stud., 1886, III., 121 ff.

and it was important that they be repeated with such changes in method as would avoid this possible criticism. This was one object in the present investigation.

In connection with these experiments Cattell investigated the number of figures, letters and words, *recognizable* after a very brief exposure. The same apparatus was used. The results are as follows : Figures, 3 to 6 ; letters, 2 to 5 ; words, 1 to 4 ; the subjects almost without exception recognized one figure more than they could letters, and one letter more than they could words. This agrees with his previous results, noticed above, by another method. The problem, however, is different from that of counting, and I need not stop to discuss the results in detail.

Numerous other investigations have been made on the recognition time of colors, words, etc., which have only an indirect bearing on the present problem and need not be mentioned here.

III. PRELIMINARY EXPERIMENTS ; HAND REACTIONS.

The problem of counting may be investigated, as we have seen, by the method of right and wrong cases ; given a short exposure (10σ) of a group of things, how large a group can be apprehended in that time so that the number is known ? This treatment of the problem can only be applied to simultaneous, or perceptive counting. It can give no help in the discussion of successive, or progressive counting, and but little in the investigation of inferential counting. A more effective method is that of reaction time. The subject reacts on the number, and the reaction times of the different numbers are compared. This avoids, for one thing, the possibility of counting from the after image. The exposure need not be so short—it should be long enough to ensure the taking in of every member of the group, and is only shortened at all in order to stimulate attention to immediate activity. In the present study the *reaction* method was adopted as principal ; but the method of right and wrong cases served as check upon the results. The times were thrown out whenever the count was wrong ; and further, if the wrong answers for a certain num-

ber equalled the right, the determination was set down as a guess rather than a count, unless the right reactions were perceptibly longer than the wrong.

Two separate investigations were made by the writer, both upon visual stimuli, but with somewhat different apparatus. The first series, carried out during the winter of 1895-6, developed a number of practical defects, which were remedied in the second series, made in the winter of 1896-7.

In the earlier series, the apparatus consisted of a large screen, with a slit 6 cm. wide and 16 cm. high, behind which swung a pendulum with a small screen attached; when the pendulum was up (and held in place by an electro-magnet) the small screen covered the slit in the larger one. The slit was on a level with the eyes of the subject, who was seated at a distance of 3 m. Behind the slit and the pendulum was fixed a holder, in which were placed, one at a time, the cards used in the experiment; this holder was of course concealed from view by the small screen when the pendulum was raised. The objects to be counted consisted of small white squares, of 5 mm.; these were pasted in a vertical line at distances of 5 mm. on the cards, which were black. In some cases the distances of the spots and their size were varied. The experimenter sat near the apparatus and was concealed (as well as the chronoscope) from the subject by another screen; he released the pendulum by means of a key. A contact was made at the point where the white spots first became visible to the subject, and the latter thereupon reacted on the number with a Morse key, at the same time calling out the number. The exposure was not limited, the pendulum being held back by a catch so that the spots remained full in view until after the reaction. By watching the (Hipp) chronoscope hands, the experimenter could tell whether the reaction preceded the speech; anticipatory reactions on the mere light stimulus were thus prevented.

Four subjects took part in these experiments, from only two of whom, however (C and G), were any large series obtained. A third (H) was unable to avoid anticipations; many of his results had to be discarded on this account, and he finally abandoned the work. The writer, who was the fourth subject (W),

acted as experimenter most of the time, in order that the other subjects might not become too familiar with the appearance of the cards. The experiments were conducted in the daylight. On ordinarily bright days the spots were easily distinguishable by the subjects, and were yet close enough together to come within the range of clear vision, so that no eye-movements were necessary to distinguish them.

The method was open to the following criticisms: (1) On cloudy days the spots were less easily discernible than on bright days; it was impossible to measure the illumination or determine the effect of its variations upon the reaction time. (2) There was found to be a tendency on the part of the subjects, after a certain amount of practice, to *judge* the number of spots by the amount of space they covered on the card, *i. e.*, the length of the broken white line which they formed. (3) While it was possible for the experimenter to distinguish anticipatory *light* reactions, in the manner above mentioned, slight anticipations could not be detected; furthermore, (4), the attention being divided between the hand and the voice, the reactions themselves might not be reliably uniform. While this last objection did not appear to the writer to be borne out by the actual results, it was obviated in the second series by the use of a mouth key for the reactions; the third objection was met by this same change. The second objection was partly met in the earlier experiments by varying the size of the spots and their distance apart; but the conditions of the apparatus prevented this from being available—or at least effective—for numbers greater than Five; with larger numbers there was no room in the slit for greater distances, and with distances less than the normal the spots were difficult to distinguish; if larger or smaller spots were used, the new cards soon came to be recognized and judged as well as the original. In spite of the defects of this method, the results obtained are of service to compare with the later ones. They are also of value in themselves in several particulars.

There were in all 40 sittings in this series, of which 19 were made by C and 9 by G; in each case two sittings were set apart for preliminary practice in simple reaction; the

results of these are not included in the tables. At the beginning of each regular sitting, before the counting reactions were begun, a series of 10 sensory reactions was taken on a card with four spots; a motor series was sometimes taken also. The subject C was of a distinctly sensory type, as these results show (Table I.) and as was proved by repeated tests elsewhere.

TABLE I.—SIMPLE REACTIONS; VISUAL, HAND, IN LIGHT.

	S	MV	No.	SER.	M'r	MV	No.	SER.
C	291.9	52.9	94	10	324.3	58.4	91	9
G	351.9	74.4	60	6	285.9	69.7	39	4
H	244.3	33.	40	4	222.2	28.3	30	3
W	235.6	58.9	43	4	179.3	40.4	21	2
C st.	192.1	34.8	50	3	247.8	58.6	30	2
C st. at.	185.9	26.9	10	1	—	—	—	—

S = sensory; M'r = motor; MV = mean variation; No. = number of reactions; Ser. = series of reactions; st. = reaction on strip of white paper; at. = reaction with great attention. The times are given in $\sigma = .001$ sec.

G and W were of the ordinary motor type; H was slightly motor. In Table I., C's first few series are omitted, as it was found that he frequently anticipated on account of a slight sound made by the pendulum in starting; this defect was remedied in all the later sittings. To determine the relation between these results and ordinary light reactions, four series were taken with a long white strip as stimulus in place of the spots; the results are given in the last two lines of the table; in one of these series (st. at.) the subject concentrated his attention to the utmost.

The reaction times on numbers are given in Table II., the sensory time for each subject being given first for the sake of comparison. The counting time for One is seen to be in every case over 100 σ longer than the sensory time. As regards the relation between the times for the different numbers, I will delay comment until the later experiments have been presented.

TABLE II.
COUNTING REACTIONS; HAND, IN LIGHT.

	C			G			H			W		
	M	MV	No.	M	MV	No.	M	MV	No.	M	MV	No.
S	291.9	52.9	94	351.9	74.4	60	244.3	33.0	40	235.6	58.9	43
I	407.4	56.1	70	523.6	96.4	30	429.4	62.2	19	497.2	112.0	9
II	415.4	60.5	77	532.9	85.6	38	419.7	74.7	30	416.3	(50.0)	6
III	481.4	77.9	89	575.6	118.0	33	466.1	77.1	23	514.7	106.6	9
IV	620.8	94.4	74	652.7	108.5	41	600.2	118.9	16	613.8	(157.0)	5
V	934.8	195.9	53	853.5	231.6	35	828.8	(426.2)	5	638.8	(138.8)	8
VI	1274.7	311.9	47	1127.9	364.2	20	1167.7	(54.7)	4	(841.3)	(402.6)	3
VII	1783.0	448.5	26	1892.4	(206.1)	8	—	—	—	(1635.0)	—	1
VIII	1901.0	(172.0)	5	2369.2	(278.7)	4	—	—	—	(1782.5)	—	2

Roman numerals signify number of spots reacted on; S = sensory reaction; M = mean reaction time.

(Table III.) Table III. shows the number and percentage of errors to the entire number of reactions; in no case (except with H) was the number of errors so great as to suggest that any other process but actual counting was used.

TABLE III.—ERRORS IN COUNTING; LONG EXPOSURE.

	C(19)			G(9)			H(6)			W(6)	
	No.	E	%E	No.	E	%E	No.	E	%E	No.	E
I	70	0	00.	30	0	00.	19	0	00.	9	0
II	77	0	00.	38	0	00.	30	0	00.	6	0
III	94	5	05.3	33	0	00.	23	0	00.	9	0
IV	81	7	08.6	41	0	00.	22	6	27.2	7	2
V	65	12	18.8	36	1	02.8	7	2	28.6	9	1
VI	49	2	04.1	22	2	09.	7	3	42.8	3	0
VII	29	3	10.3	11	3	27.2	—	—	—	1	0
VIII	7	2	28.6	4	0	00.	—	—	—	2	0

The numbers in brackets represent the series taken.

IV. EXPERIMENTS IN COUNTING, WITH MOUTH REACTION.

In the second series artificial illumination was used. A lamp giving practically uniform light was placed in a large enclosed space, within which the pendulum swung; the room was darkened. In the front side of the enclosure was an opening 12 cm. square, but a pyramidal tube extending out 35 cm. reduced the aperture through which the light could pass to 6 cm. square, and prevented its diffusion. Attached to the pendulum was a screen large enough to cover the aperture throughout the entire pendulum-swing; in this screen was a slit 25 mm. wide. The card holder was placed in front of the opening at a distance of 1.5 m., and was illuminated during 131σ when the pendulum swung; as the pendulum was held on the farther side by a catch there was but one illumination of the card before each reaction. The subject sat near the enclosure, and at a distance of 2 m. from the card; the latter was turned at such an angle (ca. 10°) as to prevent any sheen disturbance.

The cards used in these experiments were 16.5 cm. square ; the spots were (in every case) circles of 14 mm. diameter, and were placed (in the main series) at uniform distances along the circumference of an imaginary circle, so that the center of every spot was exactly 6 cm. from the center of the card. As the spots were not in line, and the distances between them varied in different cards, and as each card could be used in four different positions, the tendency to use any 'inferential' aid in determining the number was believed to be avoided; the results and the testimony of the subjects themselves confirmed this. The spots were 18, 22 and 26 mm. apart, from edge to edge, in different cards; the same card was rarely used twice in succession, and every card was turned a quarter or half way around before using again; the end spots in the row were never on the vertical or horizontal diameters of the circle; these precautions effectually prevented inferential counting. To enable the subject to fixate the card before the experiment, a very dim gas flame was usually placed near and behind it; with one subject the slight illumination of the room was sufficient to show the outline of the card, without giving any indications as to the spots. The Hipp chronoscope was used in these experiments also, but was placed in another room, thus avoiding possible distraction from the sound. The writer, who generally attended to the cards and the pendulum, gave a preliminary signal, by shouting: 'Ready;' the subject then fixed his eyes on the card, and the Hipp was started by the person in charge. The subject reacted by means of a mouth key;¹ in the counting reactions he simply spoke the name of the number into the funnel of the key. There was thus no danger of anticipation, and no division of the attention, such as occurred when the hand key was used.

At the beginning of every sitting a series of from 10 to 20 sensory reactions was taken; the remainder of the hour was occupied with the counting reactions. The principal subjects were two in number, of whom one, C, had taken part in the

¹ The mouth key used in these experiments will be described and figured in a study by Professor Baldwin, entitled 'Type Variations in Reaction Times,' which will shortly appear in this REVIEW.

former series and in many other reaction experiments. The other, T, had never before reacted on visual stimuli. The writer acted as subject in a number of sittings, and his results are included in the tables also; some reactions were made by a fourth subject, Ta, who was called away, however, before the experiments had advanced far; his results are not included.¹

There were 40 sittings in all, of which 18 were given by T and 12 by C; 10 of each included counting reactions with the mouth key. In the first six sittings with T, the hand key was used; the counting reactions made in this way are not included in the tables, but the sensory reactions are given in Table IV.,

TABLE IV.—COMPARISON OF MOUTH AND HAND REACTIONS.

		S	MV	No.	SER.	M'R	MV	No.	SER.
C	m. d.	298.3	44.7	130	10	476.8	48.7	20	1
	h. l.	291.9	52.9	94	10	324.3	58.4	91	9
	st. h. l.	192.1	34.8	50	3	247.8	58.6	30	2
W	m. d.	378.	55.4	81	5	288.1	22.8	30	2
	h. l.	235.6	58.9	43	4	179.3	40.4	21	2
T	m. d.	362.8	48.3	153	10	343.1	29.7	18	1
	h. d.	260.	23.2	75	4	250.2	31.4	59	3

All are simple visual reactions; m = mouth, h = hand reaction; d = in dark; l = in light; st = reaction on bright stimulus; cf. Table I.

for the sake of comparison; the simple reactions of C and W in the earlier series are also set down in this table along with their speech-key reactions. Of the reactions given in Table IV., only T's included mouth and hand reactions under uniform conditions of illumination; here the difference is close to 100σ, for both sensory and motor, in favor of the hand. In the cases of C and W, the hand reactions (as was observed above) include series in which the light stimulus differed greatly;

¹The writer wishes to express his thanks to all who took part in the experiments; as well as to Professor Baldwin, for many valuable suggestions on both the practical and the theoretical sides of this investigation.

hence the wide variation in the results—for C a difference of 152.5σ in the motor and of only 6.4σ in the sensory.

In Table V. the simple reactions with the mouth key are brought together; as before, the first two (practice) series of

TABLE V.—SIMPLE REACTIONS; VISUAL, MOUTH, IN DARK.

	S	MV	No.	SER.	M'R	MV	No.	SER.
T	362.8	48.3	153	10	343.1	29.7	18	1
C	298.3	44.7	130	10	476.8	48.7	20	1
W	378.0	55.4	81	5	288.1	22.8	30	2

Symbols same as in Table I.

each subject are omitted. T appears to be of a slightly motor type, while the earlier results with C and W are confirmed—they belong to distinctly sensory and motor types, respectively.

The two following tables give the counting reactions. In Table VI. the mean reaction time (M) and mean variation

TABLE VI.—COUNTING REACTIONS; MOUTH, IN DARK.

	T			C			W		
	M	MV	No.	M	MV	No.	M	MV	No.
S	362.8	48.3	153	298.3	44.7	130	37.8	55.4	81
I	567.1	83.8	23	553.1	62.6	19	573.3	75.	6
II	621.1	93.8	21	545.5	45.9	23	597.	(132.0)	3
III	655.	86.8	18	683.7	125.4	19	572.5	68.7	9
IV	683.8	123.1	42	740.4	91.2	35	588.8	(107.4)	5
V	812.3	155.8	34	1090.1	316.3	27	655.7	141.7	8
VI	938.1	154.6	17	1411.2	313.2	17	675.	53.6	6
VII	1265.	(26)	3	1352.3	362.3	6	786.8	(168.2)	5
VIII	—	—	0	(2828)	—	1	(689.3)	—	3
Zero	939.	(292)	3	831.5	(136.5)	4	717.3	—	3
Infinity	1007.7	434.	16	1128.	—	1	671.7	—	3

Symbols as in Table II.; Zero = reaction on blank card; Infinity = reaction on number too great to count.

(MV) are given for numbers from One up to Seven. The number of (successful) reactions for each number (No.) appears in a separate column. For the sake of comparison the corresponding data of the sensory reactions (S) are added also. As the mean variation is considerable, the reactions can be better compared by means of the upper and lower limits of their variation; these are given in Table VII., together with the

TABLE VII.—LIMITS OF MEAN VARIATION.

	T		C		W	
	Lower.	Upper.	Lower.	Upper.	Lower.	Upper.
S	314.5	411.1	253.6	343.	322.6	433.4
I	483.3	650.9	490.5	615.7	498.3	648.3
II	527.3	714.9	499.6	591.4	465.	729.
III	568.2	741.8	558.3	809.1	503.8	641.2
IV	560.7	806.9	649.2	831.6	481.4	696.2
V	656.5	968.1	773.8	1406.4	514.	797.4
VI	783.5	1092.7	1098.	1724.4	621.4	728.6
VII	1239.	1291.	990.	1714.6	618.2	955.

limits of the sensory reactions. From this table it appears that the counting reactions, even for One and Two, are very much longer than the simple reactions, while the difference between the times for successive numbers in every case (except T for Seven) falls within the limits of mean variation of the next. These results may be expressed under the two following propositions: (1) The shortest counting times are longer than the shortest sensory reactions by about 200σ; and (2) For successive numbers the counting time is approximately the same. Several remarks should be made on each of these statements.

As to the first: the question of the relation between counting and recognition times comes up at once. All the published experiments on recognition time having been made with the hand key, which gives decidedly shorter times than the mouth key here used (cf. Table IV.), it is impossible to compare them

directly with these results. They do admit of comparison, however, with our earlier series. Comparing the latter with Titchener's results reported in the *Philosophische Studien*¹, we find the following: Titchener gives the sensory time on light stimulus, for three subjects, as 260, 266 and 279 σ ; and the reaction time on the recognition of a word as 319.3, 317 and 302.8 σ for the same subjects. My hand reaction experiments give the sensory times of C, G, H and W, respectively, as 291.9, 351.9, 244.3 and 235.6 σ (cf., Table II.); and their counting reactions on One as 407.4, 523.6, 429.4 and 497.2 σ . The counting time is thus seen to be somewhat longer than the recognition time, if different subjects can be compared; as it happens, the writer (W) was the first-named subject in Titchener's experiments, which furnishes one case of direct comparison.

Returning to our second proposition, the following interpretation may be given: taking the mean time of counting One for standard, the subject is usually able to count Two, often Three, and occasionally Four and Five in the same time, *i. e.*, by the same kind of simple mental act. With Four or more this seems to be due to a special effort of the attention, or (occasionally) to an expectation of that particular number; in the earlier series there was some assistance from the judgment (inference), but this was carefully guarded against in the present series. In general, then, *it seems to require a longer time, and hence a more complex mental act, to count numbers greater than Three*. For Six and Seven the difference is so marked as nearly to double the length of the reaction time. With these higher numbers, too, other elements come in, as will be seen when we examine Tables VIII. and X., so that the results represent something very different from simple perceptive counting.

In Table VIII. are shown the errors (E) committed in counting each number, and the percentage of errors (% E) to total reactions.² In the two last columns for each subject the errors are clas-

¹ VIII., 138-144.

² A few reactions are included in this table, from which, through the fault of the apparatus or its operators, no reaction times were obtained, but which are available for the present purpose; this will explain the discrepancy between the figures given in Tables VI. and VIII.

TABLE VIII.—ERRORS IN COUNTING; EXPOSURE OF 1310.

	T					C					W				
	No.	E	%E	+(∞)	—	No	E	%E	+(∞)	—	No.	E	%E	+(∞)	—
I	31	0	00.	—	—	30	0	00.	—	—	8	0	00	—	—
II	34	0	00.	—	—	30	1	03.4	1	0	8	0	00	—	—
III	35	2	05.7	1	1	30	5	16.7	5	0	12	1	08.3	0	1
IV	58	7	12.1	6	1	50	4	08.	4	0	9	0	00	0	0
V	53	12	22.6	5(1)	6	50	17	34.	8	9	19	4	21.	1(1)	2
VI	51	33	64.7	17(9)	7	50	30	60.	10(1)	19	16	6	37.5	0	6
VII	21	15	71.4	3(11)	1	10	4	40.	2	2	14	4	28.5	3	1
VIII	10	10	100	1(9)	0	5	4	80.	0	4	24	18	75.	(6)	12

sified according as the answers given were too great (+) or too small (—); in some cases, it will be noticed, the subject reacted on discovering that the number was too great to count (∞); these are given in brackets in the *plus* column. It will be seen that the percentage of errors increased steadily (with slight exceptions) in the two principal subjects from Three upwards, until at Eight it reached practically 100. This explains why no reaction times are given for Eight in Table VI.¹ No cards with more than eight spots were used, owing to this fact, but the subjects did not know of this till near the end of the series; it will be noticed that T gave one Nine-reaction and nine 'Infinity'-reactions on Eight.

The conclusion to be drawn at once from a comparison of these tables is that the upper limit of counting without inference and without eye-movements is Seven or less. In T's case the number of wrong answers begins to exceed the right at Six: In C's case it exceeds it at Six but not at Seven (where only ten trials were made). Moreover, if we take into account the 'doubtful cases,' 'guesses' and 'judgments or inferences' (cf. Table X.), *the limit for progressive counting without eye-movement falls in both cases to Five.*

The two subjects differed somewhat in their method of procedure, as shown by the differences in the number of errors,

¹The bracketed numbers given there were of 'doubtful' reactions; cf. Table X.

guesses, inferences and 'Infinity'-reactions,¹ but their results agree substantially in the limits for the various kinds of counting. Although these results cannot be generalized without corroboration from other subjects, they are of great value as coming from subjects of two distinct mental types, the sensory and the motor. The distinctions which the subjects made between the different counting processes, simple perception of number, inference or judgment, guessing and progressive counting agreed substantially also; these distinctions will be explained and discussed later, in connection with the final series of experiments on inferential counting. The reactions of W are too few in number to be of much service; they present a substantial agreement with the others for the lower numbers; for the higher numbers the times are shorter and the proportion of errors far smaller; this is probably due to the writer being familiar with the individual cards from having made them and handled them in most of the experiments.

Before leaving the present question we may compare briefly the results of the mouth-key experiments with the hand-key experiments of the former series. It appears that the hand reactions are generally shorter; in the case of C, who acted as subject in both series, the difference is very uniform except for the higher numbers. In the hand reactions, it will be remembered, the number remained in view until after the subject had reacted; there was thus an opportunity for 'progressive counting,' which was taken advantage of; so that instead of guessing or inferring the number (as was sometimes necessary in the mouth reactions), the subject would take more time and 'count up' the spots. The smaller percentage of errors and the longer time required to count larger numbers, are indications of this tendency.

V. EXPERIMENTS ON INFERENTIAL COUNTING.

When the main series with the mouth key were practically completed, the subjects T and C were tested with a set of geometrical figures; for example, three spots in the form of a triangle, four in the form of a square, five in a quincunx, etc.

¹ See Table X.; cf. also Table IX., especially the results for Eight.

Of the forms used, some were regular and others irregular. The apparatus and general procedure were the same as in the main series. As the figures had to be frequently changed to avoid mere recognition reaction, there were a great many different ones used, and it is impracticable to tabulate them all. A number of typical examples are given, however, in Table IX., showing the effects of various arrangements. The num-

TABLE IX.—COUNTING BY INFERENCE.

FIGURES.	T				C			
	N	M	N'	E	N	M	N'	E
III ∴	2	784.	2	0	3	595.3	4	0
V ∴	4	836.2	8	0	4	810.	4	0
VI ∴	6	1181.8	8	1	6	1546.6	7	1
VII ∴	8	1051.1	9	0	9	1452.2	9	0
VIII ∴	0	—	4	4	3	3234.3	3	0
IX ∴	2	1108.5	2	0	3	1603.	3	0
XII ∴	2	1631.5	2	0	1	2108.	1	0

ber of successful reactions and mean reaction times are given in the columns headed N and M. The columns headed N' and E represent the whole number of attempted reactions and errors, respectively, as in Table VIII. It was found that for the higher numbers a regular arrangement facilitated the count, especially where the figure was compact; in the case of a straight line and a polygon of six sides or more the regularity rather impeded it; the count was still more impeded where the arrangement was irregular.

The fact that numbers as high as Twelve were correctly counted after so short an exposure shows at once that the process employed was different from that employed in the regular experiments. This is confirmed by the after-judgments of the subjects, who described the procedure as 'inference,' 'counting,' and 'guessing.' When these terms were explained they were found to indicate radically different processes. *Inference* was the term used when the number was judged from the shape,

etc., or inferred from the memory of the same figure as seen before. *Counting* was applied to the progressive or 'one, two, three,' counting. *Guessing* was a combination of progressive counting for part of a group, with a guess or judgment of the remainder; it is really a species of inferential counting. The counting of some numbers, such as Nine and Twelve in the table, was performed by a kind of multiplication; the subject called this process inference (or judgment), explaining at the end of the test that he included multiplication under this head.

TABLE X.—CHARACTER OF REACTION AND COUNT.

		II	III	IV	V	VI	VII	VIII
T	Whole number of reactions	34	36	59	53	55	27	12
	Errors	0	2	7	11	24	4	1
	Reactions on light	0	1	1	0	2	2	0
	Too large; no reaction	0	0	0	0	2	4	2
	Too large; reaction (∞)	0	0	0	1	9	11	9
	Inferences	0	0	0	0	3	1	0
	Guesses	0	0	0	6	8	2	0
	Counts	0	0	0	0	3	1	0
	Unspecified	34	33	51	35	4	2	0
	Doubtful	0	1	1	4	1	3	0
C	Whole number of reactions	30	30	50	50	51	10	5
	Errors	1	5	4	17	29	4	4
	Reactions on light	0	0	0	0	1	0	0
	Too large; no reaction	0	0	0	0	0	0	0
	Too large reaction (∞)	0	0	0	0	1	0	0
	Inferences	0	0	0	1	2	0	1
	Guesses	0	0	2	8	4	3	0
	Counts	0	0	0	4	8	0	0
	Unspecified	29	25	44	20	6	3	0
	Doubtful	0	0	1	3	7	2	1
W	Whole number of reactions	8	12	10	20	16	14	25
	Errors	0	1	0	3	6	4	12
	Reactions on light	0	0	1	1	0	0	0
	Too large; no reaction	0	0	0	0	0	0	1
	Too large; reaction (∞)	0	0	0	1	0	0	6
	Inferences	0	0	0	0	0	1	0
	Guesses	0	0	0	0	0	1	0
	Counts	0	0	0	0	0	0	0
	Unspecified	8	11	9	15	10	8	6
	Doubtful	0	0	1	2	4	1	4

In Table X. are shown the processes used in the main series of counting experiments (cf. Tables VI. and VIII.), as described after each reaction by the subject; inferences and guesses were always reported; a large proportion of the successful reactions on numbers higher than Four, which are not expressly ascribed to one or other of these processes (those in the row labeled 'unspecified'), are undoubtedly cases of progressive counting ('counts').¹ No attempt was made in the present study to distinguish between inference and association; in the table both processes are included under the term inference. The word judgment, which was sometimes used by the subjects in place of inference, has been generally avoided in the discussion as being too broad and indefinite. Since 'guessing,' as here used, is a complex process whose chief characteristic is an inference, this leaves but three distinct processes by which the subjects gained their knowledge of the numbers, according to their own statements, viz., the processes which we have called perceptive, progressive and inferential counting.

VI. CONCLUSIONS.

Referring back to the questions proposed at the outset, we find that definite, if not complete, answers can be given to both as a result of the present investigation.

1. *The Limit of Perceptive Counting*.—The limit of perceptive counting, with two adult subjects (T and C) one of motor and one of sensory type, both intellectually bright, but with no special talent for numbers, was found to lie at Four; this number was occasionally grasped and reacted on in the same time as One, but only of Three and Two could this be said generally. Investigations of other subjects (G, H and W, as well as C again) with hand reactions served to confirm this view. We conclude, therefore, that, *except under special stress of attention, or with subjects especially apt in this direction, the function of perceptive counting is limited to the numbers One, Two and Three.*

¹ In Table X. the rows are mutually exclusive, except the first and last; the 'whole number of reactions' equals the sum of the other rows, leaving out the row of 'doubtful' answers.

2. *The Rôle of Inference.*—To apprehend numbers greater than Four, then, some other function must come into play. The process by which this knowledge is first attained is what we have termed ‘progressive counting.’ It consists in establishing what mathematicians call a ‘one-to-one’ relation between the objects in the given group and the series of natural numbers; when the group is exhausted the last number reached in the count is known to be the number of objects in the group. But this process is comparatively slow, and in practice it is often shortened by one or another device. Thus we know by frequent experience (*e. g.*, with playing-cards or dominos) that the figure called a quincunx is a group of five things; when, therefore, we see such a figure, instead of counting the spots progressively, we *associate the number-name* (Five) with the group; and so of other figures which have become known by repeated experience. Or, again: given three rows of three spots each, although this particular figure may not be familiar to us, still we know from the multiplication table (which *is* familiar enough) that ‘three times three is nine,’ and upon perceiving the three spots on each side we immediately associate the number Nine with the group. A third case, not touched on in any of our experiments, is where the group is divided into sub-groups of various sizes; here we may count the sub-groups separately—by the perceptive or progressive processes—and reach the sum total at once through our knowledge of the addition table; this is another instance of inference based on association. Now it appears from our final series of experiments (Table IX.) that for the higher numbers the use of this inferential process shortens the reaction time, but that for the lower ones it does not—in fact, it tends rather to lengthen it. In other words, *inference tends to shorten progressive counting and to lengthen perceptive counting*, when it takes their place wholly or in part.

It would be useless to attempt to measure the *amount* of shortening produced by influence, since the time of the latter process itself varies within wide limits. In cases where we are very familiar with a certain grouping it may reduce the time enormously; in others, the inferential process is so complex that it is of little use in expediting the count. The chief result

of the present experiments, as regards the higher numbers, is to show that progressive counting is a comparatively long process, and that we must resort habitually to some kind of inference in counting large groups. Familiar figures are rare, and are practically confined to groups of less than a dozen; but addition and multiplication, combined with perceptive or progressive counting are common resources. As a matter of fact, we do not often have occasion to count very large groups; when we do, we usually fall back upon one or other of these inferential processes.

VII. SOME EXPERIMENTS ON THE SUCCESSIVE DOUBLE-POINT THRESHOLD.

BY PROFESSOR G. A. TAWNEY, AND PROFESSOR C. W. HODGE.

Beloit College.

Lafayette College.

Nearly all of the experiments on the tactual double-point threshold have been carried on by the method of least changes, the original of which was first conceived and applied by that father of experimental methods, E. H. Weber. Of the very large number of discussions in this field, which have appeared since the original discussion of Weber, only one¹ investigates the double-point threshold with successive stimuli. At the same time, it has been well known by every experimenter in this field that the threshold for the perception of successive points must be much shorter than that for the perception of simultaneous stimuli on the same spot; for the effect of any slight failure to set the two points upon the skin simultaneously is always the perception of the two points before the ordinary threshold has been reached. The following is the report of some experiments which, if not sufficiently numerous to entitle them to the claim of completeness, may nevertheless be helpful as preparatory to a more protracted study.

The object of the experiments was to determine the thresh-

¹Judd, Ueber Raumwahrnehmung im Gebiete des Tastsinnes, Phil. Stud., Bd. XII., 409-463.

old for the perception of spatial difference, and that for the direction of the difference (in eight different directions from the same spot of skin) with different intervals of time between the stimulations. Among the problems which were expected to appear in the course of the experiments were the following: (1) is the threshold for the perception of spatial difference, with successive stimuli, the same as that for direction, or is it different? (2) are they the same for all directions from the same spot of skin? (3) do they vary for different lengths of time-interval between the stimuli, and if so, according to what law? (4) is any light to be gained from these results upon simultaneous stimuli, and upon the general question as to the nature of tactual space-perception?

The subjects of these experiments were four, Professor H. C. Warren (W), Mr. J. F. Crawford (C), Dr. C. W. Hodge (H), Dr. G. A. Tawney (T). Excepting the latter none of these had any practice in the performance of such experiments. (T. had taken part in an extended series of experiments on the tactual double-point threshold for simultaneous stimuli.)

The arm of the subject rested upon the table, a screen concealing it and the apparatus from him. A piece of wood was so placed that the subject could grasp it, and thus preserve the same position of the arm during each hour. The spot investigated was also secured by marking the point on the skin which was first touched in each experiment. The temperature of the room was kept approximately constant, and the general conditions of the experiments, such as mental preoccupation, the mood and the health of the subject, the time of day, etc., were carefully noted before each hour.

The instrument used was a Verdin æsthesiometer. In order to facilitate the experiments, the instrument was suspended by a cord which passed over a pulley to a swinging weight. A difficulty arose in the determination of the distance on the arm of the second point stimulated from the first. We wished to use both points of the instrument in order to take advantage of the millimeter scale of the æsthesiometer, but the points could not rest upon the arm at the same time. One of the points was accordingly elevated by inserting a piece of wood

beneath the indicator, the other point remaining extended to the full length of the spring. A card containing a small hole and fastened to a piece of wood was so placed that by passing the points of the *æsthesiometer* through the hole successively, the same spot on the skin could be touched by the extended point and then pointed to by the elevated point. In this way the distance of the second stimulation from the first could be read from the *æsthesiometer* scale without touching the arm with both points. The hole in the card was suspended over the same spot on the skin from day to day. The experiments were conducted in the following manner: The extended point was first passed through the hole in the card with a pressure against the skin of about 50 g. The *æsthesiometer* was then raised and the elevated or shortened point was made to pass through the hole and point to the spot just touched by the other point, while the other point pressed the skin at a distance measured by the horizontal graduated bar of the instrument. This second pressure was also about 50 g. The points were of bone suitably rounded off so as not to cause pain.

To regulate the duration of the stimulations, a metronome was made to vibrate at the desired rate in an instrument case across the room. The duration of the stimulation, as well as that of the interval between the stimulations, was regulated by counting the beats of the metronome. Another precaution was found necessary with reference to the pressure of the points. By the conditions of the experiments the first of each pair of stimulations occurred at the same spot on the arm throughout the series. But the repeated stimulation of this spot gave rise, in some cases, to a qualitative difference between the sensations, which soon came to be recognized by the subject as pain. Thus the second point might be recognized as different from the first without any perception of spatial difference. This, it is true, is an inference wherever it occurs, whereas the answers of the subject ought to be direct perceptions; but he very easily, as experience proved, mistakes his inference in this case for an act of perception, and even though he should not do so, it is very probable that the inferred knowledge that the points are in fact not the same would have a pronounced effect upon his answers.

To avoid this result, we simply lessened the pressure upon the first point as the hour proceeded, asking the reagent to inform us whenever any qualitative or quantitative differences appeared between the two sensations. It has been asserted that one of the conditions of these and similar experiments is that the two sensations be subjectively the same in intensity, and it is usually assumed that this is to be secured by the same objective pressure. But one finds in fact that two points on the skin are very seldom equally sensitive to the same objective pressure. The only adequate method of securing like subjective intensities is the empirical one of testing the two spots until we have ascertained their relative sensibility.

The object of the first series of experiments was to determine the threshold for the perception of spatial difference in two successive stimulations, and also that for the perception of the direction of the second stimulation from the first. The interval between the two stimulations was a constant one of three seconds in this series of experiments. Eight directions were chosen in all, viz. up (toward the shoulder), down (toward the hand), in (toward the little finger side of the arm), out (toward the thumb side of the arm), up-out (half way between up and out), and similarly down-out, down-in, up-in. According to the method of least changes, the series in any one direction should be reversed and the average drawn from the two thresholds thus obtained. As the direction of the second point from the first is known in the reverse series of these experiments, it seemed best to separate the two series and not to follow the usual custom of taking the average between the two. The question also arose whether the thresholds for the diagonals might not be different from those for the axes, owing to the direction, and it was decided to take the thresholds for the four axes first, then proceed to the thresholds for the diagonals, and lastly to take the eight directions together. In the latter case, the eight directions could not be taken without readjusting the apparatus and, rather than do this (which would notify the subject of the direction), one of the directions, viz. up, was omitted. The experiments on H were performed by T, and those on T by H. Tables I. II. and III. show the results of the di-

TABLE I.

Showing thresholds of difference and thresholds of direction on the axes from the originally stimulated point; also middle threshold and middle variation.

HODGE.

IN.		DOWN.		OUT.		UP.		Each Day.	Aver. Thresh.
Difference.	Direction.	Difference.	Direction.	Difference.	Direction.	Difference.	Direction.		
0	6	2	2	0	6	2	4	1.0	4.5
1	14	1	6	2	2	2	2	1.5	6.0
1	9	2	3	0	2	1	8	1.0	5.7
3	3	4	4	1	7	2	6	2.5	5.0
3	20+	1	20+	3	4	3	3	2.5	3.5
2	32	3	5	3	5	5	5	3.3	11.
20+	5	3	7	3	5	4	4	3.3	5.0
4.3	12.7	2.3	6.7	1.7	4.4	2.7	4.5	M. Thresh.	
4.5	8.1	.9	4.1	1.2	1.5	1.1	1.6	M. Var.	

TAWNEY.

2	6	2	4	0	6	2	3	1.5	4.7
1	2	1	3	2	4	1	1	1.2	2.5
1	2	1	2	2	2	3	3	1.7	2.2
1	8	3	3	2	5	2	2	2.0	4.5
0	5	3	5	2	3	1	3	1.5	4.0
2	2	1	3	4	4	3	3	2.5	3.0
1.2	4.1	1.8	3	2	6	2	3.7	M. Thresh.	
	2.1		.6		2		1.2	M. Var.	

WARREN.

1	14	1	12	1	12	20+	20+	1.0	12.5
3	10	2	9	2	11	1	20+	2.0	10.0
5	12	2	8	1	8	2	4	2.5	8.0
3	12	1.6	9.6	1.3	10.3	7.6	14.6	M. Thresh.	
1.3	1.3	0.4	1.5	0.4	1.5	8.2	7.1	M. Var.	

TABLE II.

Showing thresholds of difference and thresholds of direction on the four diagonals from the point originally stimulated, the middle variation and the middle threshold.

HODGE.

DOWN-IN.		DOWN-OUT.		UP-IN.		UP-OUT.		Each Day.	Aver. Thresh.
Difference.	Direction.	Difference.	Direction.	Difference.	Direction.	Difference.	Direction.		
5	6	1	5	1	11	4	4	2.7	6.5
3	6	4	4	2	5	2	3	2.7	4.5
2	7	3	5	3	3	3	3	2.7	4.5
1	5	4	5	2	5	5	5	3.0	5.0
4	5	3	3	2	6	4	4	3.2	4.5
3	5	1	2	4	12	1	1	2.2	5.0
4	6	3	3	3	6	1	6	2.7	4.2
3	5.7	3	3.8	2.5	6.8	3	3.7	M. Thresh.	
.9	.6	.9	1.2	.8	2.3	1.3	1.1	M. Var.	

TAWNEY.

2	6	3	5	2	2	1	3	2	4
3	3	3	3	1	1	5	6	3	3.2
1	2	0	1	2	2	1	1	1	1.5
0	3	2	7	2	2	1	8	1.2	5.0
1	2	2	3	1	2	1	2	1.2	2.2
1	2	2	6	1	5	1	3	1.2	4.0
1	3	1	2	1	2	1	5	1	3.0
1.3	3	2	3.8	1.4	4	1.6	4	M. Thresh.	
.7	.8	.7	1.8	.5	2	1	2	M. Var.	

WARREN.

1	10	1	10	1	6	1	2	1	7
1	7	1	7	2	2	1	1	1.2	4.2
1	8.5	1	8.5	1.5	4	1	1.5	M. Thresh.	
0	1.5	0	1.5	.5	2	0	.5	M. Var.	

rect series of experiments on the axis, on the diagonals, and on the axis and diagonals combined, the thresholds for difference and direction being given side by side. The number 20 indicates cases where a wrong suggestion prevailed to such an extent that a correct answer was never reached.

What impresses us first on looking over the tables is the difference between the two thresholds, *i. e.*, between that for spatial difference between the two stimulated points on the arm, and that for the direction of the second point from the first. This difference has lead the writer already referred to to the conclusion that the threshold for the tactual perception of spatial difference is shorter than that for the tactual perception of spatial direction under the conditions of these experiments. But when we consider that the idea of direction is inseparable in thought from the idea of spatial difference, it seems improbable that there should be a perception of spatial difference without a perception of the direction of one point from the other. In other words it seems difficult to sense spatial difference without direction. And yet we are told that this is the real nature of all those cases where the threshold for the perception of difference is shorter than that for the perception of direction. This means that space is after all not the form of tactual perception; at least, that space in three dimensions is not.

But again, the most of the cases, upon which this inference rests, do not pretend to be perceptions of spatial difference without any direction. A direction is usually given by the subject, but it happens to be false, and the inference is drawn by the operator that a difference is perceptible, but not the direction of the difference. Is it not possible that a difference is perceived which is not spatial at all, and that the subject comes to give it the worth of real space-perception by illusion? It should be remembered that we possess an objective standard for determining the direction-threshold, such as we do not possess for the difference-threshold. In the case of difference alone, the answer is usually correct, because a difference is, as a rule, actually present; but in the matter of direction we take the correctness of the answer as a criterion of a real perception of direction. Is it not possible that there may exist an illusion

as to difference as well as to direction? After the discovery which has recently been made of the enormous part played by suggestion in the perception of two simultaneous points,¹ it is at least possible, not to say probable, that the same law works here also. Of this we can speak more advantageously later. What we are concerned here with is, first, that we have no right to apply an objective criterion of true perception in the case of direction unless we can apply the same standard in the case of difference; we should refrain from generalizing until the facts of the case have been more thoroughly looked into; secondly, this generalization, that the difference threshold is the smaller, would in no case be acceptable if it were possible to account for the observed facts by such a well established law as one which has been included under the general term, the association of ideas, but which we prefer to call suggestion. The direction given by the subject may be wrong, as it often is; but this merely constitutes a mistake of perception which, where persistent in any one direction, we call illusion.

Assuming that the apparent perceptions of difference without direction are not bona fide instances of perception in all respects, what explanation can be given for the errors in the judgments of direction? It seems as though a very natural explanation is to be found in experiences with which we are made familiar every day and hour. The perception-act in these experiments differs from that of ordinary experience in the fact that the subject is not allowed to see the spot stimulated and the instrument stimulating, at the same time he feels the touch. That constant practice of testing our tactual sensations by the sensations of a much more highly developed organ is therefore not possible here; and, consequently, the assimilation of the present impressions goes on by means of visual and motor images, as every one can easily persuade himself by trying the experiments on himself. Just as in reading we pass over typographical errors without being in the least conscious of their presence, because the actual visual images are assimilated to a correct visual image of the arrangement of the words and letters; so in these experi-

¹Tawney, Ueber die Wahrnehmung zweier Punkte, etc., *Phil. Stud.*, Bd. XIII., S. 163 ff.

ments, one assimilates the actual tactual impressions to a revival copy of similar experiences in the past, but to a copy which is actually not in accordance with the facts because the association bond between the tactual stimulus and the visual or motor image is not sufficiently close to be accurate. The local sign of the tactual sensation is, as has been said before, no simple quality of the sensation itself, but just this associational bond between the sensation and the visual or motor image to which it is assimilated. The question of most interest is, what determines the visual or motor image to be of this or that sort. One finds that both difference and direction are sometimes given when the same point has been stimulated twice in succession, that the direction given, even when the points are actually different, is often wrong, and that the error in the direction judgment seems to lie persistently in the direction of the judgments in the last series of experiments, in a direction suggested by the operator himself, or in a direction which the subject gets by autosuggestion. In some cases it is probable that more than one of these causes are present to determine the subject's answer.

Taking up the answers in which an actual difference is present between the two stimulations, but in which the direction given is wrong, they may be divided into three groups. Some follow some external suggestion, *i. e.*, they are influenced either by a previous judgment, or by the combined influence of the previous judgment and the stimulus. An illustration of the former sort would be as follows: Supposing that an up series has just been taken, and that the present series is one in which the distance increases downward, the subject gives the answer 'up.' The following would be an illustration of the latter: Supposing that an in series has just been taken, and that the present series is progressing downward, the subject answers 'down-in.' Out of 11 such wrong answers made by H during these experiments, 3 fell under the first head and 8 under the second. In the case of W, out of 13 wrong answers of this kind 9 belong to the first class and 4 to the second. T made only one wrong answer of this sort, belonging to the first class.

A second group of cases in which wrong answers were given admit of explanation as instances of autosuggestion.

From an objective point of view they seem to be accidental. The subjective process involved seems to be somewhat as follows: The subject feels the first and then the second stimulus, different in time, and at once strives to assimilate the two impressions to his past experiences. He represents the second impression in this or that direction from the first, in order to see whether the actual impression seems any different from the mental image of past experience. The tactual impression being very vague in space quality, he receives no correction, *i. e.*, the image and the impression seem the same and at once fuse into one perception. All perception seems to involve some such process as this. Every presentation is composed partly of elements of the present stimulation and partly of elements of past experience. The present sensation gives to the whole the vivid character which it itself possesses. Illusion always arises whenever the representation elements of the experience dominate over the whole so as to give it a meaning which the actual sensation elements do not possess. Of course this does not explain the cases at hand; it merely suggests a possible way in which the erroneous judgments of direction come to be given below the threshold for the perception of two points.

Another group of answers seem to be due, in one case, to autosuggestion combined with a stimulus element, and, in another case, to autosuggestion combined with the influence of the previous judgment. Of the former sort one finds in the answers of H 25 instances, and of the latter, 1; in the case of T, 37 of the former, and 3 of the latter; in the answers of W, 46 of the former kind and 16 of the latter.

Granting the hypothesis of suggestion to start with, it seems that all of these instances of wrong answers as to direction illustrate one form or another of the same process.

This conclusion seems the more probable when we consider the group of answers in which difference and direction are both given, while the same point on the skin is stimulated twice. This occurs much oftener in descending series than in ascending series, because in the former the actual direction of the second point from the first is distinctly felt in the first experiment of the series, and this knowledge operates as a sug-

gestion after the difference between the points has disappeared. In the similar experiments of Dr. C. H. Judd,¹ the smallest threshold for the perception of spatial *difference* in descending series is 0, as given in his table. All such instances are obviously due to some sort of suggestion. They correspond to the Vexirfehler in experiments with two simultaneous stimuli. In the case of ascending series the suggestion may be automatic, in descending, external, *i. e.*, from a previous judgment. In the ascending series, however, it may also be due, as above, to the influence of a previous series of judgments or of experiments. A test of this hypothesis, which seemed to be crucial, occurred in the often repeated answer of H, 'spatial difference without direction;' but upon reflecting upon the subjective process involved, he believes these judgments to be at bottom inferences, based upon slight qualitative differences in the two stimulations. No direction can be given, simply because they are inferences; were they perceptions they would be perceptions of direction, though erroneous. In the answers of W, two instances of this phenomenon are to be found. When questioned as to the subjective process involved, he replied, in the first instance, that he had not paid close attention to the first stimulus, and felt, when the second came, that it must be different because of the previous answers which he had made in the series, but that he had no idea whether the direction was the same as in previous instances or not. In the second instance he observed that a certain direction was present in his visual image of the point stimulated, but that he simply was not sure as to the correctness of the representation. Such cases did not occur with T.

A modification of this class of cases is seen in answers which indicate partial location, as, *e. g.*, where the answer is 'up or up-in,' 'out, up-out, or up.' Here the uncertainty as to the correctness of the mental representation is limited to a few alternatives. Where this occurs with H, who is a poor visualizer, the answer seems to be the result of self-questioning as to the probable direction in which the series is progressing. In the case of W, who is a good visualizer, it seems to express

¹ Loc. cit., pp. 420, 421.

uncertainty as to the correctness of the visual image, which, as he says, is usually present in these experiments. These cases also never occur with T.

TABLE IV.—SUGGESTIVE PROGRESSIONS.

Showing the number of series in which the influence of suggestion is obvious; the total number of series; the lower and upper limits of thresholds found, together with their mean; the number of single wrong answers; and the ratio of wrong answers which seemed due to suggestion to the total number of wrong answers.

	H.	W.	T.
Number of Progressions	57	21	65
Number of Series	92	21	90
Threshold { Limits	1-32	1-15	1-9
{ Mean	16.5	8	5
Number of wrong answers	442	134	199
Ratio of suggestions to wrong answers . . .	66:442	87:134	61:199

A comprehensive view of the results of these experiments is offered in Table IV. In the upper line the total number of series of experiments in which suggestive influences are apparent, is given for each of the three subjects, H, W and T. Suggestions of different kinds sometimes appear within the same series, as (*c. g.*) when, after an up-series, the subject answers, when the same point is stimulated twice, 'up,' and continues this answer until the second stimulation has reached a distance of 5 mm, in the direction downward from the first; here he answers 'up-in,' and, as the series progresses and the distance becomes greater, 'in,' 'down-in,' and finally 'down.' Here we have the influence of a previous series of experiments and judgments at first dominating, then a combination of the influence of the actual stimulus with that of the previous judgments, and finally the influence of the stimulus alone; illustrating what is called in the table a suggestive progression.

In the second line the total number of series of experiments is given for comparison with the number in which suggestive

influences appear. This line shows, in the case of H, W and T, respectively, that $\frac{5}{9}\frac{7}{2}$, all, and $\frac{1}{1}\frac{3}{8}$ of all the series were influenced in this way.

In the third line the lower and upper limits of threshold-variations, together with the mean of those two, are given to show the result of varying suggestive influences under similar external conditions. It must be remembered, however, that differences of direction are not taken into consideration in this line, and it is true that the threshold for some directions is lower than for others; other factors than suggestion, such as direction, thus come in to vary the threshold, but all other factors combined are not sufficient to account for the wide divergence apparent in these figures.

In the fourth and fifth lines are reported (1) the total number of single wrong answers occurring throughout the experiments, and (2) the ratio of those answers in which suggestive influences are apparent to the total number of wrong answers. A word should be said with reference to the wrong answers which are not referable to the influence of suggestion. These were mostly the answer 'same,' meaning that the two stimulations seem to be on the same spot. This is the answer which one expects in response to all distances which lie below the threshold. Such answers sometimes occur, however, when the distance is above the threshold, and it is possible that suggestion has played some part in these. We are not in a position to say, however, that it does so, or to what extent it enters in, owing to the absence of objective criteria. Another group of answers were simple expressions of uncertainty and suspense, and are not to be counted among the wrong answers at all. The answer, 'same,' is often given at the beginning of series, *i. e.*, when the two stimulations are really the same, and the answer is then right. The difference between the wrong answers due to suggestion and those in which suggestive influences are not apparent seems to be chiefly this, that in the former class some element of mental content suddenly makes its appearance in consciousness and influences the judgment, while in the latter class nothing arises to modify the usual reaction of the attention to the stimulus; and this difference is what is meant by suggestion.

II.

Another group of experiments was begun in which the series were all descending. In the experiments the direction was necessarily known to the subject, the object of the experiments being to determine where difference and direction cease to be perceived. Table V. shows the results. Wherever no threshold is given, the subject continued to give a difference and a direction at the end of the series when the same point was stimulated twice.

The influence of suggestion is obvious throughout this table in the fact that the thresholds, where they appear at all, are much lower than those of the ascending series. All of the subjects continued to give a difference and a direction when the same point was stimulated twice. In the answers of T, 11 are of this kind; in those of H, 23; and in those of W, 22. These answers are due, we take it, to the same influence which produces the low thresholds.

III.

The following experiments were carried out by Dr. Hodge in the Princeton Laboratory for the purpose of determining what influence, if any, the length of the interval between the two stimulations has upon the threshold for the perception of spatial difference between the two stimulations. But before going on to describe the experiments, we will notice a few facts as to the subjective processes involved, which are closely connected with the foregoing. Professor W. is a good visualizer; he always closed his eyes during the experiments, and gave close attention to the arm and the spots stimulated as they appeared in the visual copy. Both subjects were given to making a judgment of difference or of direction, or of both, at the beginning of series where the same point was stimulated twice in succession. Such errors (corresponding to the Vexirfehler in experiments with simultaneous stimuli) could, in some cases, as above, in the experiments already reported, be accounted for by the influence of the preceding series, or that of a previous judgment, but the two subjects differ very decidedly in this respect. In the case of C such judgments could, as a rule, be traced to such influences,

whereas, in the case of W they seemed, as a rule, to follow no law; they seemed to be purely accidental, so far as the outward conditions of the judgments were concerned. This difference corresponds to another which appears in the observations of W, that to him it seemed unnatural to pay attention to sensations of touch alone. He finds that it requires an especial effort to keep the tactual stimulus before the attention, while distraction from slight causes is easy and frequent. W finds it difficult to keep the first stimulus before the attention for the entire period between the first and the second, when this is 15 seconds.

All of these facts would seem to indicate that the perception of two points with W depends chiefly upon the presence of visual associations. It agrees with this that in some series his replies seem at the start to be determined partly by the stimulus and partly by autosuggestion, but as the series progresses it is not the stimulus which triumphs, as we expect, but the suggestion, and no continuance of the series will suffice to correct the persistent operation of the suggestion. Again, in some series, the presence of more than one suggestion is apparent, neither of which seems to be in any way connected with the stimulus. The visual image of the first point stimulated in each of the experiments of a given series seems to grow more and more distinct as the series progresses, showing that the difficulty in judging correctly lies, not in the absence of definite visual representations, but in the absence of the proper association links between the tactual excitations and the corresponding visual images. W seems to rely, as a matter of habit, far more on visual than upon tactual images for his knowledge of the objects with which he comes into contact. The series in which he knew nothing as to the nature of the series, whether it was along the axes or along the diagonals, were much longer than those in which he had some knowledge as to their nature from the start; showing the comparatively weak significance of the stimuli when experienced alone, it requiring much longer to recognize the direction.

With C tactual and motor images predominate; but the association between these images and the sensations is much closer than is the case with W.

C quite frequently remembers the first stimulus by the aid of a motor image of himself stimulating the point with his left hand. One notices in C also a greater tendency to use whatever data he may be able to acquire for *inferring* what the nature of the stimulus may be. He has a habit of assigning first one direction to the second stimulus from the first, and then another until he finds one which brings the series to an end; and no amount of instruction as to how the answers should be made suffices to divert this tendency. He as a rule infers that his answer is not correct whenever the series is continued for more than two or three experiments further. This is for him a constant suggestion, wherever it occurs. It was for W also in some cases, although not so habitually as with C. The latter also gets information as to the direction which probably is being taken in the experiments by remembering the directions which have already been tried. Sometimes he has the suggestion that the direction pursued is one of two or three, and proceeds by a method of elimination to go from one to another until he reaches one which brings the series to an end. Occasionally he forms an hypothesis as to the direction and answers accordingly until the sensations either confirm or contradict it, the process by which he forms the hypothesis being in some cases a purely inferential one and in other cases an associative one. In some cases, as he observes, he has no notion, prior to the stimulus, as to the direction in question, and answers according to the tactual or motor images suggested by the second stimulus. For C the tactual perception for spatial difference is always a result based upon certain qualitative differences between the sensations involved. When asked to describe the qualitative differences referred to, he speaks in terms which to us seem most vague and indefinite, and which characterize nothing, so far as we can determine, which enters into our own experience. To him, however, they have a clear and definite character. It is, moreover, significant that he observes the presence of certain tactual and motor images as an assimilating factor in every perception; how the assimilation takes place he would not undertake to say, although this is probably the same process for tactual and motor images as W describes for visual. No doubt

C's answers were quite often inferences as much as assimilations; but it seems clear that his usual method of answering is one of assimilating his present sensations to tactual or motor images of previous experiences in the perception of two points.

Going on to the experiments conducted by Dr. Hodge to determine the relation between the length of interval between the two stimuli and the threshold for the perception of two points, the following were the results: The interval was determined as before by a metronome which was placed across the room in an instrument case. The intervals chosen were 2 sec., 5 sec., 10 sec., and 15 sec. It was impossible to make the interval shorter than two seconds and preserve the conditions constant, owing to the nature of the apparatus. A longer interval than 15 sec. could not be chosen because of the difficulty involved in retaining the first sensation in memory until the second should follow. The experiments were made in three groups as follows: first, those in which the directions were straight up, down, in, or out (the axes); those in which the diagonal directions were chosen, and those in which the eight directions were all taken within the same hour. One determination was made for each of the four intervals within each hour in order to have the conditions as near the same as possible for experiments which were to be compared. The number 20+ in the following tables indicate the series in which, because of some false suggestion, the subject never succeeded in making correct answers. The o's indicate the series in which, as a result of chance coincidence, the direction hit upon by the subject when, at the beginning of the series, the same point was stimulated twice, happened to be the correct one for that series. In such series it is of course impossible to determine what the real threshold is. The direction thresholds for the perception of two simultaneous points applied in the same regions as the following experiments were as follows: For C, across, 10 mm., up and down, 15 mm., diagonal toward the thumb, 14 mm., and diagonal toward the little finger, 14 mm. For W the same thresholds were: across, 8 mm., up and down, 13 mm., diagonal toward the thumb, 8 mm., and diagonal toward the little finger, 7 mm. None but the direction thresholds are given in the

In the column headings *d* stands for down; *u-i* for up and in; *d-i* for down and in; *u-o* for up and out; *d-o* for down and out. What one first notices in the table of C's experiments is the difference between the thresholds for successive stimuli 2 seconds apart and the corresponding simultaneous thresholds. For 'across' the latter was 10 mm., while the succession threshold for 'in' is 4.5 mm., and that for 'out' 4.5 mm.; the simultaneous threshold for 'up and down' is 15 mm., while that for 'up' in successive stimuli is 4 mm., and that for 'down' 4.2 mm.; the diagonal simultaneous threshold toward the little finger is 14 mm., while the two corresponding succession thresholds are 5.7 mm. and 3.5 mm.; the simultaneous diagonal toward the thumb also is 14 mm., while the two corresponding succession thresholds are 3.3 mm. and 3.3 mm. In other words, the succession thresholds are much lower than the simultaneous ones nearest corresponding to them. But if we expect that the succession thresholds will shorten in proportion to the inverse length of the interval between the stimuli, we shall find little to confirm the suspicion. The average of all the thresholds for the interval 2 seconds with C is about 4.6 mm.; that for 5 seconds about 4.55 mm.; that for 10 seconds about 5.03 mm., and that for 15 seconds about 4.1 mm., showing a slight decrease of 0.5 mm. between the thresholds for 2 seconds and those for 15 seconds, while that for 10 seconds is considerably longer than that for either 2 seconds or 5 seconds. This is not a sufficiently definite indication to generalize upon.

Going on to Table VII., showing the results of the experiments with W, similar conclusions are to be drawn. That the succession thresholds are shorter by a very appreciable amount ($\frac{1}{2}$ to $\frac{1}{3}$) than the corresponding simultaneous is obvious. As to the question whether the threshold decreases as the interval increases, however, these experiments unite in indicating an opposite effect of lengthening the time-interval. The difficulty of retaining the first stimulus clearly in mind during the longer intervals, so marked in the case of W, may have had something to do with this result; and, on the other hand, the tendency of C to infer was no doubt assisted by the increase in the length of the interval, as it gave him somewhat more time.

TABLE VII.

Thresholds on the left forearm, volar side, of W., at intervals of 2 sec., 5 sec., 10 sec., and 15 sec.

2 SECONDS.															
AXES.				DIAGONALS.				EIGHT DIRECTIONS.							
in.	up.	d.	out.	u-i.	u-o.	d-i.	d-o.	in.	up.	d.	out.	u-i.	u-o.	d-i.	d-o.
20+	12	3	5	6	3	4	1	6	5	13	6	9	0	10	14
17	6	6	0	3	8	3	4	5	0	3	8	1	8	8	2
5	2	2	6	20+	4	3	20+	7	2	0	7	7	3	7	4
3	10	3	3	4	6	7	2	14	5	3	11	3	3	11	10
1.2	7.5	3.5	4.5	8.2	5.2	4.2	6.7	8	3	4.7	8	5	4.5	9	7.5
5 SECONDS.															
20+	5	4	11	8	1	7	2	8	2	10	4	4	8	9	5
8	5	5	5	20+	3	5	2	3	12	4	7	6	6	5	7
7	3	8	8	0	7	2	10	10	4	2	20+	5	3	3	5
4	1	2	6	6	5	5	3	4	7	2	11	2	4	0	10
9.7	3.5	4.7	7.5	8.5	4	4.7	4.2	6.2	6.2	4.5	10.5	4.2	5.2	5.6	7
10 SECONDS.															
20+	13	0	15	4	3	4	3	15	18	2	8	3	4	5	20+
5	6	2	6	10	6	3	5	18	0	5	7	3	1	2	7
8	6	3	5	8	3	2	4	6	20+	1	9	4	8	4	3
20+	3	4	6	20+	3	2	8	6	4	10	4	9	0	6	6
13.2	7	3	8	10.5	3.7	2.7	5	11	10.5	4.5	7	4.7	4.3	4.2	9
15 SECONDS.															
3	9	0	5	6	4	9	5	8	0	7	8	6	8	7	18
5	4	10	13	7	4	9	16	2	20+	7	20+	7	11	10	6
9	8	4	11	10	4	2	8	10	6	6	13	8	3	7	20+
3	3	9	4	7	12	1	3	8	4	5	3	13	6	10	1
5	6	5.7	8.2	7.5	6	5.2	8	7	7.5	6.2	11	8.5	7	8.5	11.2

So far as these two sets of experiments go, therefore, we may conclude that the threshold for successive stimuli is much shorter than that for simultaneous, but that increasing the length of the interval between the successive stimuli does not further shorten it. This may have the contrary effect. Throughout

these experiments it was observed that the same questions as to the relation of the difference to the direction threshold arose, as in the former series of experiments. The answer 'different without direction' was, however, somewhat more frequent in the experiments with successive stimuli, than in those with simultaneous stimuli; a result due, no doubt, to the suggestive effect of the succession.

Table VIII., corresponding to Table IV., offers a summary view of the part played by suggestion in this entire group of experiments. It will not be necessary to add to what has been said concerning the previous table of the same kind.

TABLE VIII. SUGGESTIVE PROGRESSIONS.

Showing the number of series in which the influence of suggestion is obvious; the total number of series; the lower and upper limits of thresholds found, together with their mean; the number of single wrong answers; and the ratio of wrong answers which seemed due to suggestion to the total number of wrong answers.

	W	C
Numbers of Progressions,	251	226
Numbers of Series,	255	255
Threshold { Limits,	1-18	1-16
{ Mean,	9.5	8.5
Number of Wrong Answers,	1509	952
Ratio of Suggestions to Wrong Answers, . .	1380 : 1509	783 : 952

There seem to be no facts in connection with these experiments with successive stimuli which do not readily harmonize with the conclusion as to the nature of our tactual perception of two points arrived at two years ago as a result of a series of experiments¹ with simultaneous stimuli in Wundt's institute, viz., that the tactual perception of two points is an assimilation process, based on association, in which visual or motor images are the assimilating, and tactual sensations the assimilated factors. We may repeat again what has been already said, that the local sign is no simple quality of tactual sensations, but rather a relation of association between the different factors, visual, motor

¹ See article in Phil. Stud. referred to above.

and tactual, which enter into the perception image. It is gratifying to find that Solomons¹ has recently come to similar conclusions in regard to the nature of the process involved. Aside from his statement that the process of reducing the threshold by practice, "as well as its general bearing on the origin of cutaneous perceptions, has been considered only speculatively" (which is not literally true), his results, so far as they go, accord for the most part with our own.

The phenomenon which, more than any other, argues against this view seems to be the answer which is sometimes given by the subject, 'different without direction.' But we have found reason for believing that this answer is either an inference from data other than tactual or a sort of illusion which arises in one or other of the following ways: either some non-spatial qualitative difference between the sensations calls up visual or motor images in which this difference appears as spatial, or some suggestion foreign to the immediate experience brings into consciousness such images, and the tactual sensations are wrongly assimilated to them.

¹Solomons 'Discrimination in Cutaneous Sensations,' *PSYCHOLOGICAL REVIEW*, Vol. IV., pp. 246-250, especially p. 248.

ON SELECTIVE THINKING.¹

BY PROFESSOR J. MARK BALDWIN.

Princeton University.

In a recent publication² I have used the phrase 'selective thinking' in a certain broad sense, and at the same time arrived at a view of the mechanism of the process which seems in a measure in line with the requirements both of psychology and biology. By 'selective thinking' I understand *the determination of the stream of thought*, considered as having a trend or direction of movement, both in the individual's mental history and also in the development of mind and knowledge in the world. The considerations suggested in the work mentioned are necessarily very schematic and undeveloped, and I wish in this address to carry them out somewhat further.

Looking at the question from a point of view analogous to that of the biologists, when they consider the problem of 'determination' in organic evolution, we are led to the following rough but serviceable division of the topics involved—a division which my discussion will follow—namely: 1. The material of selective thinking (the supply of 'thought-variations')³; 2. The function of selection (how certain variations are singled out for

¹ President's Address, American Psychological Association, Cornell Meeting, December, 1897. The paper aims to present rather a point of view, and to indicate some of the outstanding requirements of a theory, than to defend any hard and fast conclusions.

² *Social and Ethical Interpretations in Mental Development*, Macmillans, 1897.

³ Wherever the word 'variation' occurs, the full term 'thought-variation' should be understood; this is necessary in order to avoid confusion with the congenital 'variations' of biology.

survival); 3. The criteria of selection (what variations are singled out for survival); 4. Certain resulting interpretations.

I. THE MATERIAL OF SELECTIVE THINKING.

I suppose that every one will admit that the growth of the mind depends upon the constant reception of new materials—materials which do not repeat former experiences simply, but constitute in some sense ‘variations’ upon them. This is so uniform an assumption and so constant a fact that it is not necessary to enlarge upon it, at least so far as the growth of our empirical systems of knowledge is concerned. But besides the constantly enlarging and varying actual experience of the world of persons and things, we have in the reproductive functions, taken as a whole, a theatre in which seeming novelties of various sorts are constantly disporting themselves. Seeing further that it is the function of memory, strictly defined, to be true to the past, to have for its ideal the reproduction of experience without variation, it would seem to be to the more capricious exercise of the reproductive function which usually goes by the term ‘imagination,’ that we are to look for those variations in our thought contents which are not immediately forced upon us by the concrete events of the real world.

A closer approach may be made, however, to the actual sources of supply of variations in our thought contents, by taking a bird’s-eye view of the progress of thought looked at retrospectively; somewhat as the palæontologist puts his fossils in rows and so discovers the more or less consistent trend shown by this line of evolution or by that. When we come to do this, we find, indeed, certain consistent lines taken by our thought systems in their forward movement; lines which characterize certain more prominent series of stages in the descent or development of the mental life. First, we find the line of knowledges which reveal necessary fact, as we may call it; the line of correspondence between internal relations and external relations upon which Mr. Spencer enlarges, and to which the life of perception and memory must conform. Here there seems to be the minimum of personal selection, because all the data stand on approximatively the same footing, and the progress of knowl-

edge becomes mainly the recognition of reality as it is. Then, second, there is the line of development which shows the sort of concatenation in its members which goes in formal logic by the term consistency and results in some organization. This is often described as the sphere of 'truth' and belief, and is in so far contrasted with that of immediate fact. Third, there is the line of development whose terms show what has been and may be called 'fitness'—a certain very peculiar and progressive series of selections which go to build up the so-called 'ideals,' as in æsthetic and ethical experience.

In addition to these more or less selectively 'determined' lines of orderly arranged materials, there are of course multiple scattered products in the mind at all its levels; and these become especially noticeable when we cast an eye upon the outcome of imagination. We have in so-called 'passive imagination' or 'fancy,' in dreams, in reverie, in our air-castle building, untold variations, combinations and recombinations. The question which comes up for answer in this first survey of these things is this: Do the variations by which the lines of consistent, or determined, development are furthered and enriched occur as accidental but happy hits in these overproduced *disjecta membra* of the reproductive processes?

I put the question at once in this way in order to come to close quarters with a current way of looking at selective thinking—indeed about the only current way. To be sure this question of selection has not been much discussed; but those who have concerned themselves with it have generally been content to say that in imagination, broadly understood, we have the platform on which the true, the good, the valuable thought-variations occur, and from the multitudinous overplus of whose output they are selected.¹

¹ This seems to be the assumption, for example, of James (*Princ. of Psych.*, II. Chap. XXVIII). So also Dr. G. Simmel in an article (*Arch. f. sys. Philos.*, I., pp. 34 ff) which has come to my notice just as this paper goes to print; at least I find no suggestion in his article of any selection except that by movement, to which all thought-variations are alike brought through what he calls their 'dynamic aspect.' I may add that I find the general positions of Simmel on the origin and meaning of 'truth' so closely in accord with certain of the conclusions of my address that I regret the necessity of merely referring to them in the footnotes to certain of the following pages.

This, however, as it seems to me, is quite mistaken. We do not find ourselves acquiring knowledge in our dreams, thinking true in our reverie, building up our æsthetic and ethical ideals through castle-building. We do not scatter our thoughts as widely as possible in order to increase the chances of getting a true one; on the contrary, we call the man who produces the most thought-variations a 'scatter-brain,' and expect nothing inventive from him. We do not look to the chance book, to the babbling conversation of society, or to the vagaries of our own less strenuous moods, for the influence which—to readapt the words of Mr. Stout—'gives to one of our apperceptive systems a new determination.' On the contrary, we succeed in thinking well by thinking hard; we get the valuable thought-variations by concentrating attention upon the body of related knowledge which we already have; we discover new relations among the data of experience by running over and over the links and couplings of the apperceptive systems with which our minds are already filled; and our best preparation for effective progress in this line or in that comes by occupying our minds with all the riches of the world's information just upon the specific topics of our interest.

All this would lead us to a first negative position; a position which discards the view that the material of selective thinking is found among the richly varied but chaotic and indeterminate creatures of the reproductive faculty. Yet it would leave the positive answer to the question of the source of fruitful thought-variations still unanswered.

There are two alternatives still open after the view just mentioned has been discarded; one holding that it is the function of the mind to do its own determining, to think its own true thoughts, to discover the relations which are true, to bring its own forms, schemata, arrangements of parts to the manifold of sense and imagination, and so to construct its systems of knowledge by the rules of its own inventive power. This theory, it is plain, is analogous to the theory of vitalism, with a self-directing impulse, in biology; and it comes up also rather as an answer to the question as to the forms and catagories of mental determination than to that as to the material. For even though the mind have its

‘synthetic judgments *à priori*,’ as we may say in the phraseology of Kantian philosophy, still the question arises both as to the sources and as to the criteria—the local, temporal and logical signs—of the empirical data which are utilized in the forms of knowledge. I do not know that any one would be disposed to say that our knowledge of the external world, of the characters of persons, of the truths of history and natural science, are not attained through experience bit by bit; and the question to which the *à priori* theory gives no answer is: How are these bits found out? Even given the ‘catagories,’ what sorts of experiences fit the catagories, and how is the fitting done?

Leaving for a later section, therefore, the question of the origin of the categories and reverting to the only remaining real alternative, the first thing to be said is that two limitations confine us in finding the source of the variations which are available for the determination of our thinking, whatever the sphere or line of progress be. First, the new thought-variations, to be candidates for selection, are not mere stray products of fancy; but second, they are yet not outside the problem of selection from variations which arise somehow in the experience of the individual thinker. Having these two limitations full in mind, we find the third alternative—which in my own opinion all the facts go to support—to be this: *The thought-variations by the supply of which selective thinking proceeds occur in the processes at the level of organization which the system in question has already reached—a level which is thus the platform for further determinations in the same system.*

Having stated this general position, we might examine each of the lines or spheres of selective thinking already pointed out; but that does not seem to be necessary. It is just the evident difference between the child and the man, say, that the former proceeds to test data which the latter never thinks of testing. The child thinks the moon may be made of green cheese, that birds may grow on the limbs of trees, that the sun does set around the corner of the world, that eating bread-crusts does make the hair curly; such conceits the man smiles at. The difference is that at the child’s level of what we go on to call ‘systematic determination,’ these are variations of possible value; he has yet to

test them; but to the man they are not on the level or platform which his selective thinking has reached; they are not in any sense candidates for selection; they do not even enter into the complexly distributed series of thought-variations within the limits of which his criteria of value and truth lie. Various reasons have been given for this in the literature, and however they differ as explaining principles they are yet severally available as against the theory that all our imaginings afford a chance—and the more, the better the chance of profit. The untruth of this is what concerns us.

In getting his information about nature, the child learns by experimenting, as also do the animals. But having learned this or that, *he proceeds on this or that to learn more*. In judging a statement he scouts *in advance* what his lessons have already discredited. In admiring the æsthetic and in adhering to the good, he hesitates only where his sense of worth does not positively go out; what is to him ugly and bad he repudiates with emphasis.

We might take up the parable on the side of brain processes and ask what brain variations give good, true, fit, conscious states; and the same would be seen. Suppose, for example, that sane intelligent thinking over the data of the knowledge which one has already acquired involves some sort of coördination of the sensory and motor areas. This coördination is a matter of growth by integration. Variations to be fruitful—whatever be the tests of survival—must be variations in the functioning of this system. Suppose the visual center rebel and lose its coördination with the motor, or suppose the hearing center fail of its blood supply, and so drop away from the system; such changes would be gross accidents, temporary inhibitions or diseases, not variations to be selected for the upbuilding and enriching of the system. To be this, brain changes would have to take place in the delicately adjusted processes which constitute the essential coördination in question. I take this case, because, as will appear later, it suggests what is to my mind the real mechanism of thought—coördination of data in the attention, a motor function.

So far it has seemed that in each case thought-variations

must be all at a certain level if any of them are to be available for selection at that level. We may go a step further in the way of defining what is meant by 'level.'

It is just of the nature of knowledge to be an organization, a structure, a system. There is no such thing as mere 'acquaintance with' any thing; there is always—to abuse James' antithesis!—more or less 'knowledge-about.' And the growth of thought is the enlargement of the 'knowledge-about' by the union of partial with partial 'knowledges-about' in a constantly wider and fuller system of thoughts. Selective thinking is the gradual enlargement of the system, a heaping-up of the structure. If this be true, a little reflection convinces us that variations in the items of material merely, in the stones of the structure, in the brute experiences of sense or memory, can not be fruitful or the reverse for the system. It is variations only *in the organization* which can be that. It is the readjustments, the modifications or variations in the 'knowledge-about,' which constitute the gain or loss to thought. A thousand flashing colors may pass before my eyes, a thousand brute sounds make a din in my ears, a thousand personal situations flit through my imagination, a thousand reports reach me through the 'yellow journals' of the condition of Cuba; but having no tendency or force to work changes in my organized systems of knowledge, they are not even possible candidates for my selection. The rich data of the world and of history might shower upon us; the music of the spheres might tickle our ears; the ideals of the Almighty might be displayed before us in color, form and action; but, be we incapable of organizing them, they are 'as sounding brass and a tinkling cymbal.' The things of time and eternity may vary infinitely in their appeals to us, but unless *we vary to meet them* they can not become ours. So do we find actually fruitless and barren, not only the kaleidoscopic changes, the variations on variations, of our dreams and our fancy, but equally so the pages of mathematical symbols in which we have not been trained, though they embody the highest thoughts of some great genius. They do not fit into the coördinations of knowledge which are ours, nor bring about readjustments in the arrangements of them. The items, to appeal to me, must never

quite break with the past of my knowledge; each must have its hand linked with that of the thought which begot it; it must have a 'fringe' if it is to get a lodgment upon the strings of my intellectual loom and stand a chance of being woven into the texture of the carpet which is to cover the upper floor of my mental residence. The burden of mental progress, then, seems to me to lie on the side of the organizing function.

We may believe, therefore, so far as we have gone, that the material available for selective thinking is only of the sort which reflects rearrangements, new adjustments—in short, new 'determinations'—in our organized systems of knowledge; and further that each of such candidates for selection is born, so to speak, at the top of the cone, at the highest floor or level, of its own peculiar system. Other fragments of thought, *disjecta membra* of imagination, lie scattered about the bottom, unavailable and useless. With so much said about the material, we may now go on to consider the process or function of selective thinking.

II. THE PROCESS OR FUNCTION OF MENTAL SELECTION.

In the consideration of this problem—of course, the most important one—the advantages of employing the genetic method will become apparent; and it may be well to distinguish the different spheres of mental determination somewhat in the order of their original genetic appearance, the first sphere being that of our knowledge of the external world.

1. The function here is evidently one of an organization of the data of sensation in a way which shall reflect for our practical purposes, the actual state of things existing in the world. The selective process must be one which in some way concerns the active life, for it is only through the life of active muscular exertion that the appropriateness of revival processes can be tested. We have here again two alternative views which I have treated in detail in my book on 'Mental Development in the Child and the Race'; the one theory, called the 'Spencer-Bain theory,' teaching that all movements showing variation stand on the same footing, and that it is a matter of happy accident as to which of these turns out to be adaptive. Such movements so

found out are pleasurable; others, giving pain, are anti-adaptional. Through the mechanism of repetition on the one hand, and of inhibition on the other hand, the former are selected and so survive, and with them survive the feelings, thoughts, etc., which they accompany or secure. The other alternative—advocated in the work mentioned—holds that there is a difference in movements from the start, due to the conditions of waxing and waning vitality from which they spring; pleasure and pain attach respectively to these vital effects of stimulations, and so there is in each case of a selection of movements a platform or level of earlier vital adaptations from which the new variations are brought to their issue. This latter theory would seem in so far to get support from the fact brought out above, that such a platform of acquired adaptation—a level of ‘systematic determination’—is present in all selective thinking. This view holds also that such adaptive movements it is which, by their *synergy* or union, give unity and organization to the mental life.

Apart from this, however, the two theories agree in making the selection a matter of motor accommodation.¹ The system of truths about the world is a system which it will do to act upon, both when we take it as a whole and when we go into its details.

Another thing follows, however—and follows more naturally from the second of the two theories mentioned than from the first—*i. e.*, that novelty, variety, detail of experience, can be organized in the mental life only in so far as it can be accommodated to by action; if this can not take place it must remain a brute and unmeaning shock, however oft-repeated the experience of it may be. It itself, considered as a thought-variation, as well as the variations in it, would be as if non-existent—altogether without significance for the individual’s growth in knowledge. The seat of productive variations of variations, that is, from which selections are possible, must be on the motor

¹ I am not sure, however, whether Professor Bain does not here leave Mr. Spencer behind. The latter nowhere, to my knowledge, discusses selection in the sense of mental determination, but his insistence upon the direct action of the environment on an organism would seem to require him to hold that the stimulations compelled the organism to accommodate in this direction or that, the motor-selection simply coming in after the fact of determination.

side, in the active life.¹ Only thus could 'internal relations' be established which should be true to or should reproduce 'external relations.'

The point of contrast noted above between the two theories has, however, an additional interest in connection with our present topic: the point that on my theory there is a platform of earlier habitual adaptations from which the variations are always projected. For this transfers the first selective function from the environment to the organism, requires the new experience to run the gauntlet of habitual reactions or habits which organize and unify the system of knowledges, before it can be eligible for further testing by action. For example, a child can not play the piano, though he might actually go through a series of movements reproducing those of a skilled performer. The multitude of variations, so far from aiding him, is just the source of his confusion. But he can learn little by little, if he practice faithfully on the platform of the movements of the simple scales and finger exercises which he already knows how to perform.

The first test, therefore, is that of assimilation to the platform of habit. If we grant this, and also grant that subsequently to this there is a further selection, from such variations, of those which work in the environment, we get a double function of selection: *first, the sort of intra-organic selection called above 'systematic determination' which is a testing of the general character of a new experience as calling out the acquired motor habits of the organism ;² and second, an extra-organic or environmental selection, which is a testing of the special concrete character of the experience as fitted, through the motor variations to which it gives rise, to bring about a new determination in the system in which it goes.*

These selective tests we may call respectively the test of 'habit' and the test of 'accommodation to fact' (the latter

¹ By 'motor' I mean vaso-motor and glandular as well as actual muscular experiences; all of these considered as giving a reflex body of organic contents which cluster up upon incoming stimulations from the external world. It is all afferent, kinæsthetic, in its actual mechanism.

² The phrase 'intra-organic selection' suggests (intentionally on my part, although used here in a purely descriptive sense in antithesis to extra-organic) the process of adaptation called 'Intra-selection' by Weismann and described earlier by Roux under the phrase 'Struggle of the Parts.'

abridged to the test of 'fact'). These two functions of selection work together. The tests of habit, the intra-organic tests, represent an organization or systematic determination of the things guaranteed by the tests of fact; and, on the other hand, things which are not assimilable to the life of habit can not come to be established as intelligible facts. The great difference between the two tests is that that of habit is less exacting; for after a datum has passed the gauntlet of habit—or several alternative data have together passed it—it still competes for survival in the domain of fact.

What, then, do we finally mean by *truth* in the sphere of external knowledge? This, I think: a truth in nature is just something selected by the test of fact (after having passed the gauntlet of habit, of course), and then so passed back into the domain of habit that it forms part of that organization which shows the 'systematic determination' of the thinker. What the word 'truth' adds to the word 'fact' is only that a truth is a presentative datum of the intra-organic system which has stood the test of fact *and can stand it again*. A truth is an item of content which is expected, when issuing in movement, to 'work' under the exactions of fact. We speak of a correspondence between the idea and the fact as constituting truth; and so it does. But we should see that a truth is not selected because it is true; *it is true because it has been selected*, and that in both of two ways: first, by fulfilling habit, and second, by revealing fact. There is no question of truth until both these selective functions have been operative. This is to say, from the point of view of motor development, that accommodation always takes place from a platform of habit, and that in the case of the external world our first-hand knowledges arise as reflexes of such accommodations.¹

¹ In my book (*Soc. and Eth. Inter.*, Sect. 57) these two phases are generalized as follows: "With the formula: *what we do is a function of what we think*, we have this other: *what we shall think is a function of what we have done*." In general conception this is Simmel's position. In the following sentence (of which the passage in the text might almost be considered an English rendering) he is accounting for the 'Harmonie' between thought and action: he says: "Dies (Harmonie) wird erst dann begreiflich wenn die Nützlichkeit des Handelns als der primäre Faktor erscheint, der gewisse Handlungsweisen und mit ihnen die psychologischen Grundlagen ihrer züchtet, welche Grundlagen eben dann in theoretischen Hinsicht als das 'wahre' Erkennen gelten; so das ursprünglich

2. In the life of general and ideal thinking the same questions come upon us. Here we have, it is true, a certain restating of the problem, but it seems that in its essential features the principles already worked out have application. First, as to the platform—for as we saw above, thought-variations to be selected must be projected from a platform of earlier progressive thinking or systematic determination. The platform on the side of function—that is, apart from the content organized—is, I think, *the attention*. The attention is a function of organization, a function which grows with the growth of knowledge, reflects the state of knowledge, holds in its own integrity the system of data already organized in experience. I shall not dwell long upon this, seeing that it will be generally admitted, I think, that attention is in some way the organizing function of knowledge, and also because further definition—which, moreover, I have attempted elsewhere¹—is not necessary to our present purpose.

The first selection which thought-variations have to undergo, therefore, if eligibility from this platform be the first condition of final adoption, is in their getting a place in the organization which present attention-conditions represent and exact. This is just the condition of things we saw above when we pointed out that it is only the strenuous, hard and attentive concentration of mind that brings results for the life of thought. Attention is relatively easy, when we let it roam over our old stock in trade; but even then the contrast is striking between the items of knowledge which are held in the system thus easily run through with frequent repetition, and on the other hand those vestigial fragments of representation which do not engage the attention in any system of exercises, and so have no settled place or orderly se-

das Erkennen nicht zuerst wahr und dann nützlich, sondern erst nützlich und dann wahr genannt wird" (*loc. cit.*, p. 43). Simmel makes the further argument that in animals of lower orders having senses different or differently developed from ours, the motor accommodations by which the sense organs have arisen must be to different forces and conditions in the environment. So what would be counted 'truth' in the mental systems of such creatures would vary among them and also from our 'truth' (*loc. cit.*, p. 41.) An important point of difference between Simmel's view and my own is noted below.

¹*Mental Development in the Child and the Race*, Chap. XV., where it is held that the attention, organically considered, is a habitual motor reaction upon mental contents.

quence in our mental life. The latter are not *on the platform*; the former are. There is always such preliminary 'intra-organic' selection—a set of ready interests, preferences, familiarities, set to catch our new experiences or to reject them. It proceeds by motor synergy or assimilation. Thoughts which get so far in are then candidates for the other selection which the full term 'selective thinking' includes. In order to be really the thought-variations which selective thinking requires, all new items must, in the first place, secure and hold the attention; which means that they must already enter, however vaguely, into the complex of earlier knowledge, in order that the habitual motor reflex, which attention is, may be called out.

In considering in another place the empirical complex mental contents which constitute attention,¹ I found it necessary to distinguish three sets of motor events; and I threw them into a so-called 'attention formula,' as follows: *Att.* (attention) = $A + a + a$; the *A* representing the gross and relatively constant reflex effects which give attention its main sensational content; the *a* representing the special contents which vary with different classes of experience, as for example those of the different sense-qualities; and the *a* representing the refined variations which attention to particular objects as such brings out. It is a part of the general analysis of attention which issues in this formula that the state of mind called 'recognition' varies as some or other of these elements of attention are present without variation through repeated experiences. All are present without variation when we recognize a particular object as familiar; there is variation in the *a* elements only when we are able to place a new object in a familiar class but yet do not find ourselves familiar with it for itself; there are variations in both the *a* and the *a* elements when a novel experience simply meets the general requirements of our grosser life of habit, but yet has no place in the organization of our knowledge,² a state of mind characterized by so-called 'reality-feeling.'

¹*Mental Development in the Child and the Race*, Chap. X., § 3, and Chap. XI., § 2.

²Thus the animal instincts show gross motor reactions upon the objects which call them out, and it may be that the only differentiation of the objects possible to the creature is just that supplied by his differentiated instinctive attitudes.

This analysis enables us to see more clearly the meaning of the 'platform' from which thought-variations must be projected to be real candidates for selection in the life of attention. The experience which does not even bring out the constant A elements is merely a brute shock and not 'knowledge-about,' seeing that in these elements, which are necessary to all attention, we have just the gross motor adaptations in which accommodation to the external world consists. Such 'shocks' do not reach the platform.

Further, those experiences which do involve the A elements must also, at least in selective thinking, have some sort of α element and α elements with them; seeing that, in the realm of thought, attention which is not concrete involves no specific determination.¹ The study of the child shows that the differentiation of the α from the α elements is a gradual thing, the first knowledge being of a 'vaguely universal' sort (an expression of Royce's; the same thing has been called by the present writer 'the general of the first degree.') Psychologically, therefore, the platform upon which the new knowledge is to be secured is that of a sense of familiarity toward an experience, at least in the unrefined way which the child's 'vague universal' illustrates. The apprehension of a new truth is always either the consciousness of an identity, in which case it is treated as an old truth in all respects, or it is in some measure subsumed under an old truth, and so illustrates class-recognition. And it follows as to our platform that any new knowledge, to be selected and held as such, must be capable of the sort of subsumption which class-recognition is. This gives, in the sphere of general thought, the analogue of the assimilation to habit which we found necessary to the establishing of the platform of progressive determination in the case of knowledge of external objects. The two cases taken together, therefore, constitute the function of 'systematic determination.'

But this is not yet selective thinking. The selection of the

¹We often have, however, the familiar fact that the concrete content on which attention is fixed is merely a *point d'appui*, or symbol, verbal or other, which, on the organic side, merely opens a channel for the discharge of the larger wholes or attitudes (the α elements) which general and class notions presuppose.

particular concrete datum is more ; it is *an affair of the selection of variations in the attention complex*, after the datum has passed muster in the systematic determination. It is an affair of the variations of the *a* sort, at the crest, so to speak, of the attention movement. How, then, are these selected?

I think, by a precisely analogous process to that which holds for muscular accommodation by adaptation to the environment ; that is, it is a case of 'functional selection from over-produced movements.' It is here, as there, the environment's turn to get in its work, after the organism has had its turn. Yet here, as there, we must be careful to have a clear understanding of what the environment is.

The environment is here *the whole of knowledge not possessed by the individual thinker* ; that is, the whole of the social store of opinions, beliefs, reflections, judgments, criticisms, etc., within which the individual displays his reasonable activities. The selection of thoughts as valid is analogous of the selection of facts as true. Apart from the direct necessity of accommodation and recognition which the physical enforces upon us, and which constitutes the selection of certain facts from all those possible but pseudo-facts which our habitual reactions might allow to pass—apart from such physical facts, all truths are selected by a testing in the social environment, from the many pseudo-truths which have passed the gauntlet of our habitual attention reactions.

To illustrate: We see a vague outline in the dusk ; it might be a man, a beast, a tree-stump, as far as our present adaptive attitudes and recognitions avail to define it. To decide which it is and so to select one alternative as true, we put it to the physical tests of nearer approach, touch, hearing, etc. Here we have first the platform, then the selection by further action. So in thinking: We hear, let us say, a report that a friend is dead ; he may have died by accident, by poison, by fire, so far as our information goes. We find out the truth, however, by getting information from some one who knows. Here, again, is the platform with its alternatives (variations), and then the selection by a social appeal. In the case of a scientific invention the part which can be attested by an appeal to fact is so tested,

but the part which still remains hypothetical is so far liable to social confirmation that the inventor at least expects others to judge as he judges.

The use of the word 'judge' in the last sentence serves to suggest certain further considerations, which show the social appeal in operation, and, at the same time, give evidence that it is this appeal which constitutes the resource in selective thinking in the higher and more ideal spheres. These considerations may be presented under the third heading.

III. THE CRITERIA IN SELECTIVE THINKING.

By criteria here I mean not so much objective criteria—marks or characters of this or that experience—as criteria of survival, *i. e.*, the tests or qualifications which new items of experience must fulfil if they are to be given a permanent place in the organization of knowledge. This involves the question of objective criteria, to be sure; but we may be able to find some general qualification under which the special criteria of the different provinces of knowledge may be viewed. Our question may be put in the familiar terms of an analogous biological problem, if we ask: when a particular truth has been shown by selection to be such, *why was it found fit to survive?*

In answer to this question we may say at once, concerning knowledge of the external world, that the motor accommodations by which the selective process proceeds are, by the conditions of the environment, *of necessity* made in this direction or that. The reason a given movement is fit is because it actually reports fact. The dictum of the environment is: accommodate to *xyz* or die in the attempt! The facts are there; nature is what it is; the adaptations are such just because they are fit to report a state of facts. The environment in which the accommodations take place, and to which they constitute adaptations, is the control factor, and its facts are just the only reason that the selections are what they are. The criterion here, therefore, is simply the adaptive aspect of the movement, as reporting fact. It can be determined in each case only after the event; that is, after the selection has taken place.

But even in this lower sphere, where the exigencies of the

physical environment are the control-factor in the selective process, we find the further result that the preservation of the fact selected depends upon its having already been assimilated to the organized habits of the individual. As knowledge it becomes part of a system; it is added to the platform from which subsequent selections are made; and it thus carries forward the 'systematic determination' of thought. In this way *the organism gradually reproduces, in its own platform of determination, the very criteria of selection at first enforced only by the environment.* We should expect to find in consciousness some general coloring due to the attitude which the systematic determination platform requires—an attitude of welcome, of hospitality, of endorsement, in short, of *belief*—toward those facts which have passed through the selective processes, have been added to the organization of knowledge, and have acquired the *cachet* of familiarity.

I need not stop to argue that it is right to apply the term 'belief' to this sense of the internal fitness of experiences after their selection; the implied converse proposition, *i. e.*, that belief is a motor or active attitude, has been ably argued by Bain, James and others. I have advocated it in my book on 'Feeling and Will.' But whatever we call it, there is the fact—and that is what I wish to emphasize under whatever name—that even in our knowledge of nature, the individual *gradually builds up internally* the criteria of selection; and as his experience extends ever more widely afield from the brute resistances, strains, and contacts with things, he becomes a more and more competent judge for himself of the value of variations in his thoughts. Here is what is essential in it all: The sense of values has grown up all along under the actual limitations of control from the imperative selective conditions of the environment, and if one make use of his criteria of selection beyond the teachings of his experience it is only by means of those general rules which are implicit in the systematic determination itself.¹

¹ Such as the laws of identity (motor habit), consistency (motor assimilation), sufficient reason (accommodation to the item selected), etc. Cf. *Mental Development in the Child and the Race*, Chap. XI., § 1. By these I think it possible to account for the so-called 'analytical processes,' which have to deal with relationships inside the whole of systematic determination; on which see further below.

Turning now to the great platform of attention, we find an analogous state of things, and the analogy really turns out to be identity of process, thus providing a strong argument for the view that the social criterion of selection is here the true one.

In the first place, we have to recognize that *in all thinking whatsoever as such*—even in our thinking about the external world when viewed not as motor accommodation, but as a system of organized truths—*the environment is social*. For we may ask: What does environment mean? Does it not mean that set of conditions which runs continuously through the individual who is said to be in the environment? The physical environment is such because its conditions are those of motion, and the organism moves. The environment of thought can only be thoughts; only processes of thought can influence thoughts and be influenced by them. The sources from which spring items in the world of thought are ordinarily centers of thought—minds, either one's own or some one's else. So the environment must be the persons about the thinker. They constitute his environment; they give him conditions to react upon; *they are the control-factor in his higher selective thinking*, just as the world of things is the control-factor in his life of sense-perception. I know that it is through their life of action—mainly indeed, the speech-functions—that he realizes their thought, and it is through *his* life of action that he reacts upon their thought and exhibits his; but even in knowledge of the external world of signs, expressions, etc., we have to say that movement must be reduced to some form of thought in order to be organized in our knowledge. And as soon as we get out of the sphere of knowledge of the world of things, and ask how knowledge can proceed without the selective control of physical fact upon movement, we have to say that if selection is to have reference to any environment at all it must have reference to an environment of thinking. Apart from theory, however, the social life is as a matter of fact the environment of our thinking; in my recent book on 'Social Interpretations' I have cited much evidence to show that the child organizes his thoughts with constant reference to the control which the social environment enforces.

So we have found that each group of thought-variations, to be candidates for selection, must be projected from a platform of acquired knowledge, represented on the motor side by certain elements in the attention-complex which give the sense of familiarity, class-identity, general truth or vague universality. This is the platform of systematic determination through the attention. Now, why not stop here? Because when a new thing comes, this does not suffice to secure those more refined elements of the attention-complex which determines a new concrete fact. On the contrary, many alternative determinations, all of them answering the demands of the platform of vague generality, might be forthcoming and the mind might rest in any one of them. Note the child's long continued and fanciful speculations about the simplest events in the household. What must now be had is just the selective control of an environment in which such variations can be brought to a test; and to the child this is the environment supplied by the persons who know more than he does. To them he normally appeals, almost invariably accepts their decisions, and finds certain of his alternatives thus selected, by what is to him as direct an adaptation to fact as are the selections of his movements by accommodation to that other environment, the world of things. Every new piece of knowledge needs this confirmation just in so far as the systematic determination by which it is brought to the bar of selection leaves the concrete filling of the event indefinite; that is, in so far as various alternatives or variations might be brought into selective rivalry with it.

But then—and this is the vital fact in the growth of the individual—this selection by a social criterion *becomes personal to the learner through his renewed action*. The selected functions, with their knowledge contents *are added to the organization within, so that the 'systematic determination' of the future is influenced by the assimilation of each new selected element*. Thus the inner attitude which the individual brings to his experience undergoes gradual determination by the continued selective action of the social environment. He himself comes more and more to reflect the social judgment in his own systematic determination of knowledge; and there arises within him-

self a criterion of a private sort which is in essential harmony with the social demand, because genetically considered it reflects it. The individual becomes a law unto himself, exercises his private judgment, fights his own battles for truth, shows the virtue of independence and the vice of obstinacy. But he has learned to do it by the selective control of his social environment, *and in his judgment he has just a sense of this social outcome.*

In the work referred to I have dwelt at length upon the actual facts of this educative dependence of the individual upon social lessons. The aspect to be emphasized here is the selective aspect, *i. e.*, the truth that the internal criterion is, so far as it goes, always in fact the primary criterion in our thinking; but that in its origin the relation is quite the reverse; and, further, that the individual's judgment is liable all the time to the final selective revision of the social voice. This shows itself most markedly in those ideal states of mind in which the direct control of objective fact is lacking and where the private determination is more or less explicitly accompanied by a sense of 'publicity'—a sense that the public judgment is implicated with one's own in the approval or disapproval of this act or that. In our ethical judgments I think this ingredient is unmistakable.

It remains only to say again that in the state of mind called belief, mental endorsement, and in particular cases judgment, we have the actual outgoing of this systematic determination upon the details of experience. All judgments in experience are, I think, acts of systematic determination, acts of taking up an attitude, of erecting a platform from which new things, to be eligible for selection, must be viewed. The details of organization, thus gradually built up, show the relationships of our theoretical thought; and these relationships are valid since they show just the motor organization which has accrued to the attention-complex. The data of fact or objective truth are the items which have passed through the selective ordeals.¹

¹ "So erzeugen sich für unser Denken, gemäss dem Nützlichkeitsprincip, gewisse Normen seines Verhaltens, durch welche überhaupt erst das zustande kommt was wir Wahrheit nennen, und die sich in abstracter Formulirung als die logischen Gesetze darstellen" (Simmel, *loc. cit.*, p. 45.) It is here that I may note the difference between Simmel's view and my own. He makes (so far as the

The general conclusions which the sketchy development so far made would suggest may be stated in summary form before we go on to note some further points of interpretation in the last remaining section. These conclusions are as follows: Selective thinking is the result of motor accommodation to the physical and social environment; this accommodation taking place in each case, as all motor accommodation does, from a platform of earlier 'systematic determination' or habit. In the sphere of the physical environment as such, the selection is from over-produced movements projected out from the platform of the habitual adaptations of the members brought into play; in the sphere of the social environment it consists in the accommodation of the attention, secured by the over-production of motor variations projected from the platform of the habitual attention complex. The presentations from which the selected motor variations issue are believed or called 'true,' while the organization which the motor complex gradually attains holds the data of knowledge in relations of theoretical and analytical 'validity.' In the case of physical selection the internal organization represented by systematic determination gradually serves to free the organism from direct dependence upon the control of the environment;¹ in the intellectual life this is even more true, the development of the individual's judgment growing more and more independent of social control as progress is made in the 'systematic determination' which organized knowledge exhibits.

This general sameness in the operation of selection in the two spheres is what we should expect if the method of motor ac-

undeveloped form of his article justified an interpretation) the function of movement that of giving 'truth' to thought-variations *already present*. The 'dynamic aspect' in its issue secures the selection of the ready-formed 'presentative aspect.' This I hold to be true (when supplemented by the 'systematic determination' of the variations on a platform) of presentative data, wholes, or facts as such. But there still remain the determining effects of the motor-selections themselves upon the systematic determination. The synergies, inhibitions, etc., of the new motor accommodations with old habits produce changes in the *organization or relationships of the data* and give rise to theoretical and analytical 'validity' in our knowledge, which differs (as Simmel himself points out, and as Urban has independently suggested) from the objective 'truth' of given data or 'wholes.'

¹ This is seen in psycho-genetic evolution in the rise of memory, thought, etc., considered as variations.

commodation be what I have described in 'Mental Development in the Child and the Race' as the imitative or 'circular' reaction. For it is just through reactions of this type, with the antithesis between pleasure and pain by which they are furthered and maintained, that motor accommodations are all the while passed over to the domain of habit—that is, are integrated in the system of 'intra-organic determinations.' Thus organized knowledge in all its development may be looked upon as due to the *synergies* of motor processes selected as accommodations to the world of things and persons.

IV. SOME FRAGMENTARY INTERPRETATIONS.

In the way of showing certain general bearings of the position now taken—bearings which the limits of this address do not enable me to amplify in any detail—I may go on to indicate the points which follow. They are suggestions toward a broader union of psychological with biological and philosophical points of view.

I. It will be seen that the position now taken up preserves what may be called the 'utility' criterion of survival through the whole progress of knowledge. The acts of selection are never independent of control from experience, however adequate they may be *within* this control; for the internal or systematic determination, while always the platform of variation, is yet never the final agent of concrete selection. To be sure, the individual's judgment, his sense of reality and truth, becomes more independent or self-legislative, as we have seen; but this, when genetically considered, is both the outcome and the evidence of the control which the environment has all along exercised. Even though we assume certain innate norms of selection which the individual directly applies, still these norms must not only lead to workable systems of knowledge in the world of active experience; but they must in their origin themselves have been selected from variations, unless, indeed, we go back to a theory of special creation with preëstablished harmony.¹ But if we

¹ Cf. an article on *The Origin of a 'Thing' and its Nature*, PSYCH. REV., II., 1895, p. 551 (printed also in *Princ. Contrib. to Psychology*, I., No. 3, p. 105) for some criticisms of this theory.

admit that they are themselves selected variations, then we find no way to account for their selection except that by accommodation to the physical and social environments.¹ This preserves the utility criterion, therefore, even though we may not accept the precise method of selection portrayed above.

2. This does not tend, however, to give support to Mr. Spencer in looking to 'race-experience' for the origin of the categories of knowledge. Spencer's theory has been admirably criticised by Professor James, who thinks that the forms of knowledge must be looked upon as variations, not as accumulations from the repeated impressions of the environment. In support of James's argument we may add, what to me seems an insurmountable objection, that Spencer's position requires the transmission of such impressions by heredity²—a notion which James was one of the first to combat and a claim for which there is no evidence whatever. The position developed above assumes variations, with constant systematic determination in the individual's experience; in this the control of the environment is reflected. We then need a theory of evolution which will account for the determination of race-progress in the lines thus marked out by the individual.

3. This requirement is met, I believe, by the theory of Organic Selection, recently proposed by the present writer, considered as supplementary to Natural Selection in the way of securing lines of determinate evolution. According to this view, those individuals which successfully accommodate to the environment live and keep alive through heredity the congenital variations which they exhibit. To these are added further congenital variations which are again selected. Thus variations are secured in definite lines in a series of generations—lines which reproduce the determinations first secured in the individ-

¹ Simmel makes the analogous argument (*loc. cit.*, p. 45) that even if we had on a *à priori* stock of knowledge, a selection of movements would still have to be made for practical life, and a system of 'truths' would be built up thereby.

² Cf. the dogmatic utterance of Wundt *apropos* of instinct: "The assumption of the inheritance of acquired dispositions or tendencies is inevitable, if there is to be any continuity of evolution at all. We may be in doubt as to the extent of this inheritance; we cannot question the fact" (*Hum. and An. Psychology*, p. 405). Hoc atque anno 1892!

ual under the control of the environment.¹ On this view, there would be a constant selection of individuals by natural selection, from a platform of organic selection which is analogous to the platform of 'systematic determination' in the individual. Race evolution would thus, on the whole, conform to the exigencies of experience, and would seem to be directly controlled by the environment, while due, in truth, to a series of variations accumulated by Organic and Natural Selection.²

4. Furthermore, the content of the intellectual and social environment is kept constant by the handing down of tradition through 'social heredity,' and the same demands are thus made upon the individuals of each new generation, both as to their concrete selections under the control of the environment and as to the forms in which the 'systematic determination' of knowledge is cast.

5. Finally, the 'systematic determination' of the individual thinker is, on the subjective side, just his *sense of self*. Judgment is the *personal* endorsement of the data of knowledge. Belief is a *personal* attitude. The person is the whole of the organization of knowledge; and as the social criteria of selection—and the social data selected—play an essential rôle in the process of systematic determination, as explained above, so the person is a social outcome. This I have developed at length elsewhere.³

¹ Cf. the summary in the *PSYCHOLOGICAL REVIEW*, IV., p. 393, article on 'Determinate Evolution,' where references are given especially to the writings of H. F. Osborn and C. Lloyd Morgan, who arrived at the same position independently. The theory of 'Organic Selection' has been indorsed by such psychologists as Wm. James and J. McK. Cattell and by such biologists as Alfred Russell Wallace and E. B. Poulton.

² Professor Poulton says, referring to Organic Selection: "These authorities justly claim that the power of the individual to play a certain part in the struggle for life may constantly give a definite trend and direction to evolution, and that although the results of purely individual response to external forces are not hereditary, yet indirectly they may result in the permanent addition of corresponding powers to the species. * * * The principles involved seem to constitute a substantial gain in the attempt to understand the motive forces by which the great process of organic evolution has been brought about."—*Science*, October 15, 1897, p. 585.

³ In '*Social and Ethical Interpretations*.'

STUDIES FROM THE PRINCETON PSYCHOLOGICAL LABORATORY.

VIII. A STUDY OF THE TEMPERATURE SENSE.

PRELIMINARY REPORT.¹

BY J. F. CRAWFORD.

This study was carried on in the Princeton Laboratory during the year 1896-97. To call out temperature sensations brass cylinders were used 9 cm. long and $1\frac{1}{4}$ cm. wide, tapering at the last 12 mm. on each end. Two pairs of cylinders were used; in one pair the ends are $\frac{1}{2}$ mm. across, in the other considerably less. More minute work can be done with the latter pair; but the pressure must be very light to prevent pricking, and the stimulus becomes correspondingly weaker. A still sharper point will not work. A needle point, for example, even at extreme temperatures, calls out no sensation but pricking (or burning). Any finer exploration than can be made with these cylinders therefore seems impracticable. A pair of cylinders is used, the one not in use being placed in water of the desired temperature, and the two being frequently exchanged. The temperature is thus kept nearly constant. Around each cylinder are two belts of cork, which serve conveniently as handles, and prevent the cylinder from heating or cooling rapidly

¹As Mr. Crawford's work is necessarily suspended for the time this brief account of his method and results is now published. The results are so opposed to those of earlier investigators, while the method has such evident advantage over theirs (the exact reexploration of each skin-field, made possible by the device of transparent transfer-frames) that their importance, if confirmed, is at once evident. Mr. Crawford has abundant records, to my mind, to show that sensitiveness to temperature goes by areas and not by spots; for when the transfer-frames of the same area are superposed (this cannot be done by any other method) the results are: (1) clear agreement in the successive experiments as to the separate grouping of cold and hot regions respectively, and (2) a filling in of the area for either cold or hot over the whole surface. This latter would seem to be impossible were the 'spot' theory true.

J. MARK BALDWIN.

through contact with the hand. Slight changes of temperature are insignificant, especially with cold, because a difference of even 5° C. makes no practical difference in the reactions. From 0° up to 23° or 24° there are cold reactions, intense below 6° or 8° , and growing less marked above 15° . At 22° hot reactions begin to come in, but are not marked till 40° or 42° . At between 49° and 54° the heat passes over into pain, which arises from all points of the skin alike. The range of cold reactions it is seen is much greater than that of hot. No tests were made below 0° .

When a response to temperature stimulation is given, the place is marked with ink made of diamond dyes, and applied with a fine hard-wood toothpick fixed in a penholder. With this arrangement it is possible to make a dot as small as necessary and to locate it very accurately. In order to get a *permanent* record of the results after a region has been explored I use what I call a 'transfer-frame.' This is like a window of architect's paper on a frame of cardboard of convenient size (1 by $1\frac{1}{2}$ inches inside and $\frac{1}{2}$ inch wide). The results of each sitting are exactly traced on one of these transfer-frames, and this permanent record can be compared precisely with any or all other records. With the use of these transfer-frames the skin can, after each sitting, be washed clean, and a new exploration can be made without reference to old markings. The importance of this will be noticed below. In order to make results exactly comparable small identification marks are tattooed on the skin and copied with each record on a transfer-frame. With every such record of course there is kept a written record of all the conditions of experimentation, points of interest, etc.

The chief object of the work so far has been to determine whether the sensitiveness to temperature is found only in minute spots or in continuous regions, and in either case whether any topographical law of distribution could be discovered. I have had six subjects, three of whom gave me about thirty sittings each, and three about eight. Among the first three I include myself. The result of nearly 120 sittings has been less definite than was hoped; but enough has been done to establish a method of procedure, to indicate some conclusions, and at least to show the inadequacy of some of the generally accepted work.

There is much room for skill in the manipulation of the experiments, and therefore a large personal factor in their results; a factor which can be ruled out only by repeated and painstaking experimentation. Steady hand and eye are needed. The force of suggestion can be eliminated for the subject by keeping one set of cylinders in hot water and another in cold, without letting the subject know in any given case which is being used; and for the operator by repeating the experiments *de novo* and comparing records afterwards. Results do not emerge mechanically, but at best reliance must be placed on the skill and care of the operator.

To draw the cylinder along the surface of the skin, even slowly, gives unsatisfactory results, because there are misleading effects both of inertia and of after-images, because the subject's attention is distracted by the motion, and because a comparison of temperatures is rendered difficult. It is better to set the cylinder down on successive points, advancing it in parallel lines. The points of contact can be made as close together as the cylinder end allows. The result, of course, must appear in the form of dots, and may seem to beg the question at issue. But this can be determined by the degree of correspondence of different records made on the same territory discovered by the superposition of the transparent transfer-frames of the same region upon one another against a light background.

The results of these experiments indicate clearly that the sensitiveness to temperature appears not in groups of minute spots, separated by a non-sensitive background, but in *continuous regions* of varying size and indefinite limits. Within such sensitive regions are often smaller regions of greater intensity. But these never appear in the form of a group of spots. They are continuous so far as they extend, and variations of intensity are more or less gradual.

The evidence for this conclusion rises chiefly out of a comparison of records made from the same region. From the nature of the method the single records appear in the form of groups of dots; but, while in the different records the groups of dots correspond very closely, the individual dots do not correspond at all. Within a fairly sensitive region it is impossible to find any spots that are non-sensitive.

Since these conclusions are directly opposed to those of Goldscheider,¹ who, nevertheless, cannot be suspected of careless work, some explanation is demanded of how he could arrive at his conclusions. Any one following out with care the method sketched above will, I believe, soon make the matter clear to himself, though it is not easy to explain it without actual manipulation. Since any method of recording gives the result in the form of dots, and since slight errors on the part of both operator and subject are constantly involved, the result of a *single sitting* will be a definite arrangement of dots in lines and groups. If now the experiment is repeated *with the old markings on the skin*, which, if I understand him, was Goldscheider's method, one is irresistibly led to follow out the old tracks, and to find approximately the same dots as before, when one ought really to find different dots in the same group. But a method by which one begins the second series *de novo* frees one from the thralldom of the old marks, and a comparison of the results shows that Goldscheider's assumption of minute persistent points in a non-sensitive field must be replaced by that of continuously sensitive regions. The same objection may be made to the results of Kiesow,² and a different objection to those of Donaldson³ (on the ground of continuous motion and variations of pressure). My results are much closer to the original results of Blix. Dessoir⁴ discovered the incorrectness of Goldscheider's topography, but his sweeping denial of all variations of topography is absurd and has been fully met by Goldscheider himself.

I do not yet feel justified in stating any other conclusions as proved. In the size, arrangement and relative intensity of regions I have found no law. The relation between 'hot' and 'cold' seems to be one of mutual independence. They are neither coincident nor complementary, but seem to overlap without law. But these and other questions need to be much more fully pursued. Especially difficult and important is it to determine how long the effects of inertia and after-images last, and

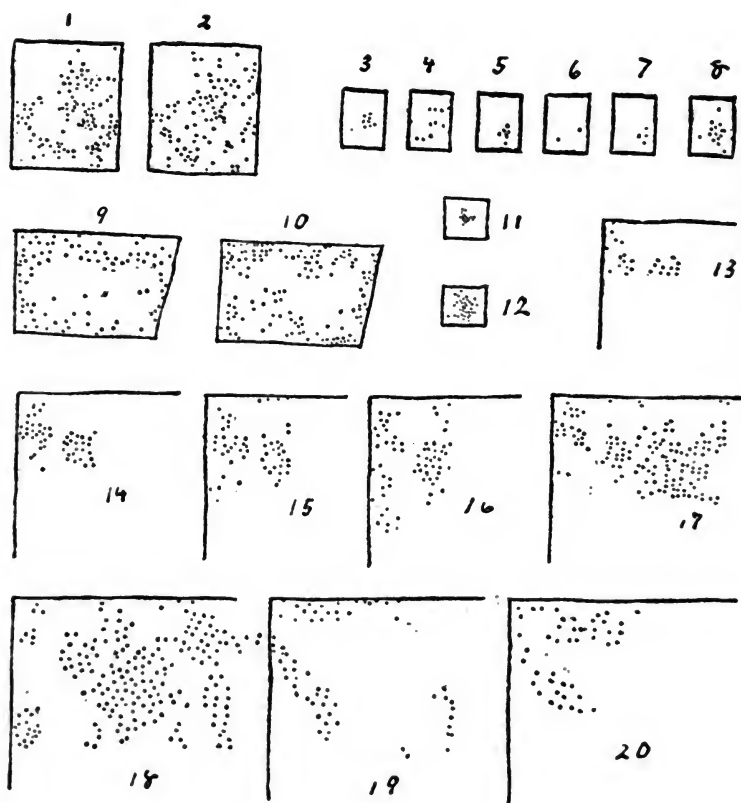
¹ Du Bois-Reymond's *Archiv*, 1885. *Suppl. Bd.* 1-110., *et al.*

² *Phil. Studien.*, XI.

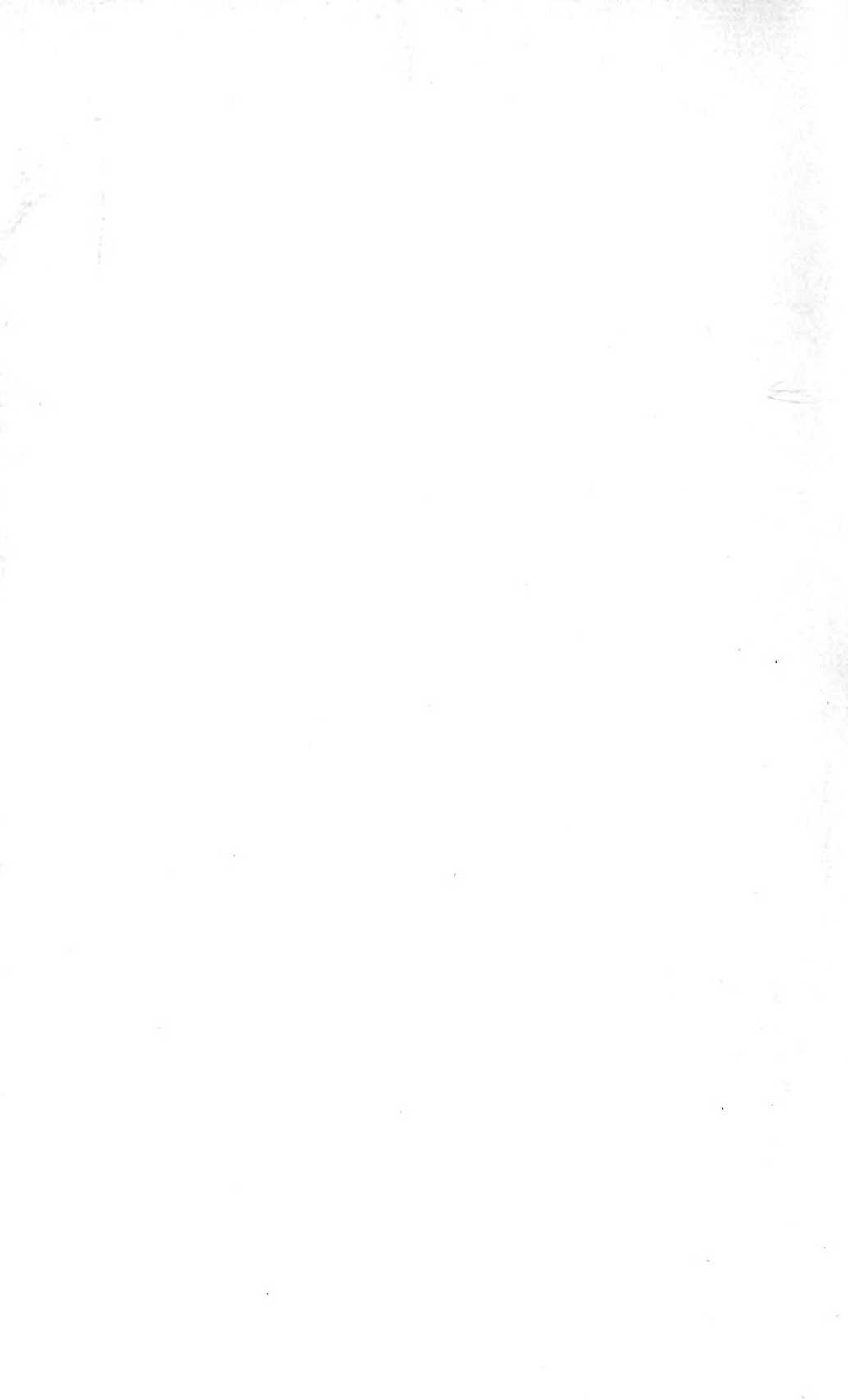
³ *Mind*, X., 399-416.

⁴ Du Bois-Reymond's *Archiv*, 1892, 175-339. See Goldscheider's *Criticism*, *Zeitschr. f. Psych. u. Phys. d. Sinnesorgane*, 1893, 117-122.

whether they extend beyond the point of actual stimulation. Till this is determined records cannot be very sure. Later I hope to give a report of more extended work, accompanied by plates containing detailed evidence taken from the record-frames, and by a full discussion of literature. The plate accompanying this paper is merely illustrative.



Explanation of plate. (1)-(2). Two explorations for cold, same region, on *K*. (3)-(8). The same 'hot' region at six sittings, on *K*. (9)-(10). Cold, repetition on same region, on *M*. (11). Attempt at minute exploration. It is not possible to find non-sensitive regions *between* these dots. (12). Similar. Both on *M*. (13)-(18). Same region on *C*, cold, but exploration becoming more extensive. (19)-(20). Same region, but with *insensibility* to cold marked. These should be nearly complementary to the others. Note that by this method groups of dots represent continuous regions, not isolated points.







Princeton contributions to psychology
v.1¹-2

BF 21 p8

**University of Toronto
Library**

**DO NOT
REMOVE
THE
CARD
FROM
THIS
POCKET**

Acme Library Card Pocket
LOWE-MARTIN CO. LIMITED

HANDBOUND
AT THE



UNIVERSITY OF
TORONTO PRESS

